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ESSAYS ON GERMANIC RELIGION

EDGAR C. POLOMÉ
University of Texas, Austin

CONTENTS

INTRODUCTION
INDO-EUROPEAN COMPONENT IN GERMANIC RELIGION
SOME COMMENTS ON *Vǫluspá*, STANZAS 17-18
DIVINE NAMES IN INDO-EUROPEAN
GERMANIC RELIGION: AN INTRODUCTION
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CONTENTS

HUMAN SKIN COLOR: THE SEXUAL DIFFERENTIATION OF ITS SOCIAL PERCEPTION
By Peter Frost 3
SEX DIFFERENCES IN CRIMINALITY: AN EXPLANATION BASED ON THE CONCEPT OF R/K SELECTION
By Lee Ellis 17
DETERMINANTS OF STATE PUBLIC WELFARE EXPENDITURES: AN ANALYTICAL SURVEY
By Robert G. Mogull 39
THE 'DEMOGRAPHIC PROBLEM' IN ISRAEL
By Daniel R. Vining, Jr. 65
CAN A MATHEMATICIAN BECOME A SOCIAL SCIENTIST? THE CASE OF WILLIAM SHOCKLEY
By J. W. Jamieson 71
INDUS WRITING
By Subhash C. Kak 113
INDO-EUROPEAN PSYCHOLOGICAL CONCEPTS AND THE SHIFT TO PSYCHOPHYSICAL DUALISM
By Noel W. Smith 119
CAN ARCHAEOLOGY AND HISTORICAL LINGUISTICS COEXIST? A CRITICAL REVIEW OF COLIN RENFREW'S *ARCHAEOLOGY AND LANGUAGE - THE PUZZLE OF INDO-EUROPEAN ORIGINS*
By Mark R. Stefanovich 129
PICTURE OUT OF FOCUS: COLIN RENFREW'S *ARCHAEOLOGY AND LANGUAGE - THE PUZZLE OF INDO-EUROPEAN ORIGINS*
By Michael Everson 159
BOOK REVIEWS 174

Cover Picture: Seal from Mohenjo-Daro depicting what later came to be known as a "Brahmin Bull." (See article: *Indus Writing*)

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HUMAN SKIN COLOR: THE SEXUAL DIFFERENTIATION OF ITS SOCIAL PERCEPTION

Peter Frost
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In all human populations, women are lighter in complexion than men. It is proposed that this sexual dimorphism led to selection pressures on women to react more positively to darker complexions. Data from Japan and North America is presented to support this hypothesis. It is unclear, however, whether sex differences in skin color preference reflect a hormonally modulated behavior or simply arise from the different learning environments in which children are reared. What prediction, then, can be made from the "hormonal hypothesis" that would not be expected from the "sex-role conditioning hypothesis"? The first hypothesis would predict that skin color preference varies with adiposity and not sex in children less than 7 years old. In this age group, ovarian secretion is minimal and estrogen is produced mainly through aromatization of androgens in fatty tissues. To test the above prediction, 225 preschool children were asked to choose between two dolls slightly different in complexion. Subjects choosing the darker-colored doll were found to be significantly fatter than those choosing the lighter-colored doll.

Human skin color varies not only with race but also with sex. In all populations, women are paler in complexion and men ruddier and browner, this sex difference being due to female skin having less hemoglobin and melanin (Frost, 1988; van den Berghe and Frost, 1986; Edwards and Duntley, 1939). The sexes differentiate in complexion at puberty, primarily as a result of genetic influences (Omoto, 1965; Kalla, 1973; Kahlon, 1976; Mesa, 1983). Age also exerts an effect independently of sex, for infants are much less pigmented than older age groups (Kahlon, 1976; Mesa, 1983; Darwin, 1874: 580-581).

Frost (1988) and Guthrie (1970) have argued that lighter skin coloration is one of several infant traits that the adult female body has retained in order to inhibit aggression in adult males and possibly to elicit care-taking behaviors. In

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other primates, the hair color, facial traits and behavior patterns of infants have been shown to elicit care-taking responses in adults and inhibit aggressive acts normally made by one adult to another. In primates where hair color differs between the sexes, this situation arises from the infant's coloration persisting in the adult female; apparently as a result of selection pressures created by the dominant mating system. Monogamy, although the rule in only 18% of all primate species, characterizes 63% of those species where the sexes differ in hair color. Under monogamy, the male invests more in its offspring, cohabits with the female for a longer time, and thus exposes her to a higher risk of death or injury from male aggression. It is apparently this increased risk which generates the selection pressures responsible for the adoption of infant traits by the female.

In our own species, infants are less pigmented owing to their greater need for Vitamin D to metabolize the calcium necessary for rapid bone growth. This trait – given the visibility of human skin – could then have acquired the secondary function of modifying adult behavior toward infants, thus creating the possibility for women to adopt it as a means to reduce aggression in adult males.

If sexual dimorphism in human skin color is, in fact, a case of infantile mimicry, it should have set in motion selection pressures for men and women to evaluate skin color differently. Women who react less aggressively to the darker complexions more typical of men would tend to form more stable pair bonds and thus have more progeny. With each passing generation, women would come to perceive and evaluate darker skin coloration more favorably; the neural processing of this sensory information would encounter increasingly higher thresholds for the release of aggressive behavior and lower ones for the stimulation of libido and other areas of sexual response.[1]

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1. In other mammals certain preferences have been shown to vary with sex. Female rats, for instance, prefer sweet solutions much more than males. This preference disappears following ovariectomy or during the altered endocrinal states of pseudopregnancy

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Studies on Adult Men and Women

In "The Social Perception of Skin Color in Japan," the sociologist Hiroshi Wagatsuma noted that Japanese men and women manifest different preferences toward skin color:

> Contemporary Japanese men interviewed in the United States and Japan all agreed in valuing the "whiteness" of skin as a component of beauty in the Japanese woman. Whiteness is very often associated in their minds with womanhood ("Whiteness is a symbol of women, distinguishing them from men"), with chastity and purity ("Whiteness suggests purity and moral virtue"), and motherhood ("One's mother image is white").
>
> With only a few exceptions, the women interviewed voiced the opinion that Japanese women like light-brown-skinned men, seeing them as more masculine than pale-skinned men. Many women distinguished between "a beautiful man" and "an attractive man." A beautiful man (*bidanshi*) is white-skinned and delicately featured like a Kabuki actor. Although he is admired and appreciated almost aesthetically, he is, at the same time, considered somewhat "too feminine" for a woman to depend upon. There is sometimes a reference to the saying, "A beautiful man lacks money and might." On the other hand, an attractive man (*ko-danshi*) is dusky-skinned, energetic, masculine, and dependable. Women often associate light-brown skin in a man with a dauntless spirit, a capacity for aggressive self-assertion, and a quality of manly sincerity.

North American studies appear to reveal a similar pattern, although such attitudes are invariably cast in racial terms. In psychoanalysis carried out on southern White women, the American psychologist Helena Deutsch found marked sexual attraction to Black men (Dollard, 1949: 170). Another social psychologist, J.L. Moreno, observed an analogous phenomenon when studying homosexual relationships between adolescent

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and lactation. It reappears if estrogen and progesterone are administered (Goy and Goldfoot, 1973; Wade, 1976).

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girls in a small American town. Although "crushes" also occurred between girls of the same racial background, such relationships differed in kind and in number from those involving a difference of color:

> But it [the crush] is different and without parallel in a normal environment when white girls have crushes on colored girls, not only as individuals, but *en masse*. This form of crush greatly outdoes in intensity, variability of attitudes, and effect upon conduct the fancy of a white girl for another white girl. It is a paradoxical phenomenon.
>
> This bizarre form of behavior we have rarely found to be mutual. It is a one-sided attraction of the white girl to the colored. The white girl goes through all the gestures of courtship, sends notes, makes dates, and tries to keep the mysterious conduct secret. Her mind is deeply preoccupied; she neglects school studies, work, and even the thought to run away. As long as it lasts, she is as if in a trance. Later, after it has faded out, she laughs when she thinks of it and cannot understand why she acted so "foolish." Sometimes two white girls work in pairs, one being the letter carrier for the other and "in" on the secret. Nothing seems to deter them – neither the housemother's criticism nor rebuffs of other girls who despise and publicize their conduct.
>
> The negro girl is taken by the white as *a substitute for a boy*.[2] This interpretation is corroborated by what the girls write and say themselves about it. (Moreno, 1934: 230-231)

Studies on Children

A study performed on subjects from 3 to 8 years of age suggests that women's greater responsiveness to darker complexions may originate in childhood. Asher and Allen (1969) asked their subjects to choose between two puppets: one

2. Italics in the original text.

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white in facial color, the other medium brown. More girls than boys chose the darker colored puppet, this difference being significant in both Black and White subjects. In Black children the sex difference in color preference tended to widen with age, as males increased in white puppet preference while females decreased.

This sex difference has been observed in a similar study conducted on 3 to 7 year old Bantu children in South Africa. When presented with two dolls differing only in pigmentation, the girls consistently preferred the darker one after being asked "Show me the doll you like best", "show me the doll that is a nice doll", and "Show me the doll that has a nice color" (Gregor and McPherson, 1966). Other doll studies, however, have not observed any sex difference when limited to subjects six years old and younger (Renninger and Williams, 1966; Williams and Roberson, 1967).

This age effect may result from the different learning environments in which boys and girls grow up. As children mature they come to learn consciously or unconsciously that certain physical traits typify the opposite sex and, as such, ought to be seen as sexually desirable. Since learning tends to be cumulative, boys and girls ought to show a steady divergence with age in their evaluation of skin color.

Alternatively, the age effect may result from increasingly divergent hormonal states acting on a behavior open to hormonal input. Whereas in early childhood boys and girls show approximately the same levels of androgens and estrogens, they increasingly in hormonal production from seven years of age onwards (Nathanson *et al*, 1941; Winter, 1978: 196). women's greater responsiveness to darker skin coloration may thus stem from a hormonally influenced behavior.

Testing for a Hormonal Influence

What predictions can be made from the "hormonal hypothesis" that would not be expected from the "sex-role conditioning hypothesis"? As already noted, the hormonal state of children below seven years of age is more or less sexually undifferentiated. At this age, the adrenal cortex produces most of the

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body's sex hormones, whose levels are then modified by the aromatization of the androgen *androstenedione* to the estrogen *estrone* in the fatty tissues of the body (Siiteri and MacDonald, 1973; Nimrod and Ryan, 1975; Perel and Killinger, 1979; Frisch *et al.*, 1980; Deslypere *et al*, 1985).[3] If sex hormones do influence preferences toward lighter and darker complexions, one would expect children having more fat, and thus more estrogen, to show more "feminine" preferences. Conversely, leaner children ought to show more "masculine" preferences.

When ovarian secretion is minimal, adiposity can be used to estimate estrogen levels in the body. In post-menopausal women, plasma estrogen significantly correlates with fat mass; the value of "r" being .72 (Vermeulen and Verdonck, 1978, 1979: Judd *et al.*, 1980). This correlation has also been observed in men and even premenopausal women, although in the latter case ovarian secretion partly obscures it (Kley *et al.*, 1980a, 1980b; Zumoff, 1982; Kirschner *et al.*, 1982). In young girls, production of estrogen in fatty tissues may explain why fatness is associated with early onset of puberty (Frisch and McArthur, 1974; Frisch and Revelle, 1970).

Frost and Lamontagne (1988) used measures of adiposity on a sample of preschool children to determine whether fatness in this age group was associated with a more "feminine" response to skin color.[4] A sample of 225 children, 96 girls and 129 boys, was drawn from eight daycare centers in the Quebec City area. All but ten of the children were of French-Canadian descent.

The children were tested individually, either in a separate

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3 Kirschner *et al* (1982) state that estrogen produced in premenarchal girls arises via extragonadal aromatization of androstenedione. In an unpublished study conducted on normal children of both sexes, Dr. Kirschner found that normal conversion rates of androstenedione to estrone could account for all the estrone excreted in their urine (Kirschner, 1999). Hemsell *et al* (1977) have also observed aromatization in normal boys, as well as in a boy who had developed severe feminization as a result of abnormally high aromatization of plasma androstenedione to estrone.

4. In this study, we followed essentially the same procedure used by Clark and Clark (1947), Asher and Allen (1969), Kircher and Furby (1971) and other child psychologists in this field.

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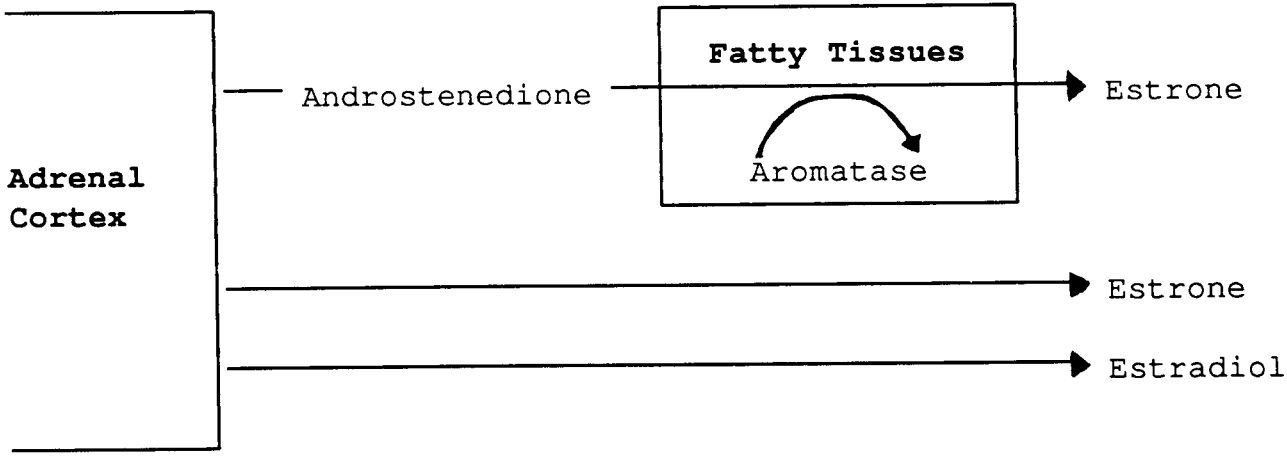
Figure 1
Estrogen Synthesis in Children



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room or in a separate corner away from the others. An assistant, unfamiliar with the aims of the research project, measured the child's height, weight and thickness of triceps fat. The child was then seated in front of two dolls placed side by side, one slightly darker in complexion than the other, and asked to pick the "nicer" (*plus gentille*) doll.[5] The child pointed to the "nicer" doll or refused to choose. The test over with, the positions of the dolls were interchanged before bringing in the next subject.

After all testing was completed, two additional estimates of fatness were derived using the following formulae: Triceps fat/height and weight/height. In both cases, the inverse square of height was employed to correct for differences in body size (Frisancho and Flegel, 1982).

Children choosing the darker doll were significantly fatter than those choosing the lighter one (see Table 1). Triceps fat was significantly thicker in the first group, the probability of chance being less than 10%. When corrected for the effects of differences in body size, this measure of adiposity showed an even greater contrast between the two groups of children. All other variables measured in the study [6] – sex, age, height, and weight – had no significant effect on doll preference. Finally, adiposity appeared more strongly associated with doll preference in younger subjects. Below 3 years of age, a third measure of adiposity – the body mass index – showed a significant difference between children choosing the darker doll and those choosing the lighter one.

---

5. The dolls were otherwise identical. As the difference in color was barely perceptible, both would have been readily identified as "White". The dolls were female, had black hair-pieces which covered their eyes and were dressed so as to expose their limbs and face to view.

6. The results revealed a more or less equal preference for the two dolls. Children younger than 3 years of age seemed to prefer the darker-colored doll more, but the difference was not significant. Younger children were also less likely to choose one doll or the other. In fact, only three subjects less than 20 months old made a choice.

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Table 1
Relationships Between Doll Preference and Measures of Adiposity

D = children choosing darker complexioned doll
L = children choosing lighter complexioned doll
p = probability of chance

	ALL AGE GROUPS				
	D		L		p
	Mean	S.D.	Mean	S.D.	
Sex (% Females)	49.5	-	44.3	-	.578
Triceps Fat (mm)	10.67	2.16	10.26	2.03	.09 *
Fat/Height^2	1.12	.33	1.03	.27	.033 **
Weight/Height^2	1.6	.15	1.59	.15	.34
	BELOW 3 YEARS OF AGE				
Triceps Fat (mm)	11.06	2.16	10.28	2.14	.132
Fat/Height^2	1.44	.33	1.28	.31	.059 *
Weight/Height^2	1.69	.14	1.6	.17	.035 **

NOTES:

* - probability of chance less than 10 %
** - probability of chance less than 5 %

Value of p determined by Chi^2 analysis for % Females.
Student T tests used for all other variables.

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Conclusion

The evidence suggests that darker complexions are perceived more favorably if the level of estrogens relative to androgens in the body rises above a certain threshold; less favorably if it falls below. Such a mechanism would produce behaviors that complement the differing complexions of men and women, the suggestion being that the environmental substrate upon which this mechanism evolved and took form was our species' sexual dimorphism in skin color.

Nyborg (1984) states that sex hormones may influence behavior in three different ways: (1) by prenatal priming of sensitivity to pubertal sex hormones, (2) by early permanent "organizational" effects on developing neural tissues, and (3) by transient "activational" effects on the functioning of mature nervous tissues. The last hormonal pathway appears to be the most likely candidate for the mechanism that seems to influence differing evaluative responses to darker and lighter complexions. Accordingly, everyone would possess both "male" and "female" behavioral repertoires that provide the appropriate responses to skin color. These behavioral repertoires, however, would be differentially activated by different ratios of estrogens to androgens in the body.

What avenues are there for further research? In the above study, measures of adiposity proved to be an inexpensive and relatively non-invasive method for estimating estrogen levels in large numbers of children. In theory, blood or urine tests can now be used to corroborate the above findings. These latter methods, however, pose serious design problems, particularly when working with young children (*i.e.* fear or shyness on the part of the subjects, difficulty involved in collecting all urine excreted over a 24 hour interval, etc.). As well, the margin of error in estimating estrogen concentrations may not necessarily be less than that incurred by using measures of adiposity.

Another possible avenue of research would be to conduct a similar study on adult women, using photos instead of dolls, to see whether skin color preference varies with the menstrual cycle. Premenopausal and postmenopausal subjects could also

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be compared in the same way.

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SEX DIFFERENCES IN CRIMINALITY: AN EXPLANATION BASED ON THE CONCEPT OF r/K SELECTION

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An r-selected approach to reproduction emphasizes producing large numbers of offspring with little parental effort to assure survival of the offspring after they have been discharged from what tends to be a short gestation period. At the other end of the continuum, K-selection emphasizes producing very few offspring, while committing great time and energy to their gestation and care, thus maximizing each offspring's reproductive viability. This report extends previous theoretical arguments that criminality reflects an r-selected approach to reproduction by hypothesizing that males are more r-selected than females, and thus are more prone toward criminal behavior than females. A literature review supported the hypothesis by indicating that, not only among humans, but throughout the mammalian order, males tend to exhibit r-selected characteristics to a greater degree than females. Results were interpreted as suggesting that a scientific explanation for sex differences in criminal behavior has now been formulated in terms of gene-based evolutionary theory.

Sex Differences in Criminality

In all societies and time periods ever studied, males have been found to exhibit higher rates of criminal behavior than females [Ellis, 1988], and the more serious and persistent the offense patterns being considered, the more males tend to dominate [Wilson & Hernnstein, 1985]. As summarized in Table 1, a number of explanations have been offered for this observation. While they are not all entirely separable form one another, the explanations can be seen to include seven that are more or less purely social in nature, and five that either focus strictly on biological variables,or, more often, on interaction between biological and social variables.

Supporting the conclusions of Eme [1979: 583] and Wilson and Rugsby [1975:43], Table 1 indicates that the most popular

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scientific explanations have been in purely social environmental terms. These explanations attribute essentially all of the sex differences in criminality either to sex role training or to sex differences in socialization and socioeconomic opportunity. Despite the popularity of purely social environmental explanation, they have been hard pressed to explain why societies have never been found in which females exceed males in the commission of serious crimes (especially in recent years when weapons have all but eliminated any physical strength advantages males may have had over females for committing crimes [see Pollak, 1950]).

Biosocial (and purely biological) explanations, on the other hand, are quite compatible with the universal nature of sex differences in criminal behavior. However, they have offered no account of the considerable variation from one society to another in the *extent* to which males dominate in the commission of crimes [Nettler, 1978:123]. Here a new biosocial explanation will be offered, one that implies that sex differences in serious criminality will vary from one society to another, and that, within a given society, sex differences in criminality may even vary somewhat over time.

An Overview of the r/K Selection Concept

MacArthur [1962; MacArthur & Wilson, 1967] introduced the concept of r/K selection to describe a continuum along which all organisms (or, more precisely, the genes they carry) seem to function. In the most general terms, r-selected organisms gravitate toward producing large numbers of offspring with little or no care given to each offspring by its parents after being discharged from a short gestation period. At the other end of the continuum, K-selected organisms normally confine themselves to only a few offspring in a lifetime; but they typically devote inordinate time and energy to offspring nurturing and care, thereby maximizing each offspring's reproductive potential.

At the most extreme K-end of the r/K continuum, some organisms may have even evolved tendencies to avoid directly reproducing all together, proliferating the genes they carry

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TABLE 1
Explanations Offered for Sex Differences in Criminality

EXPLANATIONS	PROPONENTS
Social Environmental	
Greater male opportunity to commit crime	Arnold & Brungardt, 1983:80; Giallombardo, 1982:46; Simon, 1976, 1979; Willis, 1981
Sex differences in role training, socialization, and societal expectations	Bartol, 1980:281; Cohen, 1955; Gora, 1979; Harlan, 1949:740; Haskell & Yablonsky, 1974:67; Hoffman-Bustamente, 1973; Hurwitz & Christiansen, 1983:274; Mann, 1984; McKissack, 1975:47; National Advisory Commission, 1973; 346; Pollak, 1950:123; Schur, 1969:43
Oppression of women, or the failure of women to be liberated/emancipated	Adler, 1975:42, 1976; Austin, 1982:427; Bowker, 1978; Deming, 1977:74; Konopka, 1966; Lopez-Rey, 1979:409; Nettler, 1978:123; Parmelee, 1920:244; Schur, 1969:43; Scutt, 1978; Sutherland & Cressey, 1966:139
Tendencies of parents to rear females in more protected and supervised environments	Bonger, 1905; Giordano, 1978:130; Haft, 1974:2; Hoffman-Bustamente, 1973; Lefkowitz, Heusmann & Eron, 1978; Nye, 1958:99; social Sutherland & Cressey, 1970:127
More of a need or desire by males to prove their masculinity (particularly if reared in a female-dominated household)	Parsons, 1947; Silverman & Dinitz, 1974
Sex discrimination by the criminal justice system in responding to crime	Mannheim, 1965:704; Morton, 1976:32; Pollak, Price, 1977; Reckless, 1940:98
Biologically-Based	
Unspecified biological and instinctive factors	Arzyle, 1964:67; Thomas, 1927
Sex differences in physical strength and constitution	Exner, 1949:144; Lombroso, 1918:184; Tait & Hodges, 1962:93
Genetic factors, primarily located on the Y-chromosome	Cowie, Cowie & Slater, 1968; Slater & Cowie, 1971
Neurohormonal factors	Ellis, 1987b; McKissack, 1977:259; Moyer, 1974; Silber, 1981:30; Yeudall, 1980
Natural selection pressure	Alexander, 1979:241; Cairns, 1986:75; Wilson & Daly, 1985

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merely by helping others with whom they share genes (especially close genetic relatives) to rear offspring. Such so-called *inclusive fitness* (as opposed to *reproductive fitness*) should be associated with a wide diversity of altruistic behavior patterns among K-selected species [Hinde, 1986].

Indicators of r/K Selection

Among the traits that are considered at least rough indicators of r/K selection are the following:

Numbers conceived. The greater the number of offspring conceived, the more organisms are said to be r-selected.

Gestation length, miscarriages, and neonatal mortality. The less time spent gestating offspring before discharging them to a more or less independent existence (even if rarely viable), the more r-selected organisms are said to be.

Sexual maturation. Organisms that sexually functioning relatively early are considered more r-selected than those that are slow to begin sexual functioning.

Fertility Potential. Being capable of reproducing in large numbers is an indicator of r-selection.

Copulatory frequency. R-selection tends to be associated with relatively frequent copulations, particularly with numerous sex partners and little partner selectivity.

Sexual bonding. The formation of stable alliances between sex partners in order to cooperate in rearing descendants is seen as less common among r-selected organisms than among K-selected organisms.

Parental investment. The tendency by parents to invest a great deal of time and energy in the care of each offspring is minimal for r-selected organisms and maximal for K-selected organisms.

Life expectancy. Because they contribute little to the reproductive process beyond insemination, r-selected organisms tend to have shorter life expectancies than K-selected organisms.

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Not listed as an indicator of r/K selection here, although it has been fairly often identified elsewhere as such, is body size [*e.g.*, Pianka, 1970; Daly & Wilson, 1978:125; Gould, 1982:342]. The relevance of body size has been questioned, especially for comparing within a limited spectrum of life forms, both in terms of birth weight (or egg size) [see Stearns, 1976:31] and in terms of adult body size [Ellis, 1987:153; also see Lewin, 1988:514].

r/K Selection and Victimful Criminality

Both Rushton [1985] and Ellis [1987] have recently proposed that victimful criminal behavior may be an expression of r-selected tendencies among humans. While recognizing that the human species is extremely K-selected relative to the entire spectrum of life forms, these proposals have suggested that significant intra-species variability is likely to have evolved. If so, within the range of human variability (all of which would still be confined to the K-selected end of the continuum), one would expect to find groups of humans who vary in their tendencies to help fellow group members to reproduce in ways that diminish their own reproductive potential (i.e., display unreciprocated altruistic behavior). Assuming that altruism and criminal behavior (especially violent and property offenses) tend to be at opposite ends of a single continuum [McKissack, 1975:48; Ellis, 1990], criminality should be found among humans who exhibit all, or at least most, of the above listed r-selected characteristics.

The above arguments were seen as helping to explain a number of well documented observations in the criminological literature. In particular, studies throughout the world have shown that such variables as large family size and unstable marital relationships are consistently related to involvement in serious and persistent victimful criminal behavior [reviewed by Ellis, 1983, 1988]. Also persons who commit serious victimful crimes appear to exhibit the following r-selected traits to greater degrees than persons in general:

(a) shorter periods of gestation (premature births) [Douglas, 1960; Douglas & Ross, 1968],

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TABLE 2
Evidence that Males Exhibit r-Selected Traits to a Greater Degree than Females*

r-Selected Trait	Among Humans	Among Species of Nonhuman Primates	Among Species of Nonprimate Mammals
greater numbers conceived	Cavalli-Sforza & Bodmer, 1971; Hnevkovsky, Petrikova & Cerny, 1964; Hyttend & Leitch, 1971; McMillen, 1979; Mikamo, 1969; Momoli & Volet, 1962; Morrow, Cupp & Sachs, 1965; Scheinfeld, 1971; Ser & Ismajovich, 1963; Slader & Bang, 1969; Stevenson, 1959; Szontagh, Jakobovitz, & Mehes, 1961; Teitze, 1948; Tricom, Serr, & Solish, 1960; Wagner, 1958 [Klinger, et al., 1958; Moore & Hyrniuk, 1960; Sasaki, et al., 1967]	Clark, 1978; Izard & Simons, 1986	Chapman, Casida & Cote, 1938; Gardner, 1950; Johansson, 1932 [Johansson, 1932; Johansson & Venge, 1951]
shorter gestation period or more miscarriages, & neonatal mortality	Ciocco, 1940; Gadpaille, 1972: 201; Geschwind & Galibruda, 1985: 547; Hammond, 1965; McMillen, 1979	Izard & Simons, 1986	Mead, Gregory & Regan, 1949 [Brakel, et al., 1952]
more rapid development to sexual functioning	Asayama, 1975; Clement, Schmit & Kruse, 1984; Hass, 1979; Huntor & Jessor, 1975; oore & Erickson, 1985: 42; Reiss, 1967; Schofield, 168: 49; Sorenson, 1973; Vener, Stewart, & Hager, 1972	Abbott, 1978; Kraemer et al., 1982; Nadler, Herndon & Wallis, 1986: 376; Nieuwenhuijsen, et al., 1988:367	
higher fertility potential	Betzig, 1986; Chagnon, 1979; Daly & Wilson, 1982; Dickemann, 1979; Irons, 1979; Thornhill & Thornhill, 1983: 155		Bateman, 1948; Gallup & Suarez, 1983; Hartung, 1982: 1
more frequent copulations, especially without pair bonding, or a stronger preference for such (stronger sex drive)	Asayama, 1975; Bromley & Britten, 1938; DeLamater & MacCorquodale, 1979; Dreyer, 1982: 569; Hamilton, 1929; Hersch, 1988: 34; Hunt, 1974; Kinsey, et al., 1953; Kirkpatrick, 1959; Mellen, 1981: 192; Moore & Erickson, 1985: 42; Newcomer & Udry, 1985; Owuamanam, 1982; Porterfield & Ellison, 1946; Symons, 1979; Terman, 1938; Zelnik & Shah, 1983 [Clement, Schmidt & Kruse, 1984]	Keverne 1976; Kraemer et al, 1982:400; Michael & Zumpe, 1970; Wilson, 1972	Beach, 1947:264; Trivers, 1972

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unstable bonding with sex partners	Engels, 1972:72; Freedman, 1979: 78; Kinsey, et al., 1953; Lockard & Adams, 1981: 185; Mellen, 1981: 270; Sigusch & Schmidt, 1971		
lower parental investment made in the offspring	Alfaro, 1981; Benedict, White, & Cornely, 1985; Gelles, 1978; Gil, 1970; Kaplun & Reich, 1977; Lightcap, Kurland & Bergess, 1982 Maden & Wrench, 1977; O'Neill, 1973; Welsh, 1976 [Birdsell, 1953; Nathan & Hwag, 1981; Wilson, Daly & Weghorst, 1981; Veel, 1970:816]	Itani, 1983; Levine & Doering, 1974: 79; Mitchell, 1968; Mitchell & Brandt, 1970	 [von Saal & Howard, 1982]
lower parental investment made by the parent	Brandon, 1976; Burke, 1984: 31; Jason & Andereck, 1983; Mackey, 1985; Whiting & Edwards, 1973; Rhoades & Parker, 1981 Taylor, et al., 1978 [Bennie & Sclare, 1969; Maden & Wrench, 1977; Parke & Collmer, 1975; Straus, Gelles, & Steinmetz, 1980]	Chamove, Harlow & Mitchell, 1967; Mellen, 1981:35; [Gibber, 1981]	Davis & Gandelman, 1972; Fisher, 1930; Gandelman, 1972; Richard & Sculman, 1982: 239; Rosenberg, et al., 1971; Rosenberg & Sherman 1975; Rosenblatt, 1967; Svare & Gandelman, 1973 [Elwood, 1977, 1980; Jakubowski & Terkel, 1982; Marques & Valenstein, 1976; Rowell, 1961; Svare & Bartke, 1978; Svare, Bartke, & Gandelman, 1977; Yardeni-Yaron, 1952]
shorter life expectancy	Breuer, 1982:198; Ciocco, 1940; Dublin, Lotka, & Spielgelman, 1949; Gee & Veevers, 1983; Graney, 1979; Haines, 1977, 1979; Hamilton, 1948; Hamilton & Mestler, 1969; Harrison, 1978; Herdan, 1952; Johnson, 1977; Keppel, 1981; Kitagawa & Hauser, 1973; Klebba, Maurer, & Glass, 1974; Leviatan & Cohen, 1985; Lopez, 1983; Madigan, 1957; Metropolitan Life Insurance Co., 1971; Nathanson, 1978; Newsholme, 1924:204; Noble, 1978:325; Parkhurst, 1956; Potts, 1970; Preston, 1976; Rasmuson, 1971; Rao, 1973:412; Retherford, 1975; Scheinfeld, 1958; Sowder, 1954; Stolnitz, 1956; Thomson & Geddes, 1931: 568; U.S. National Center for Health Statistics, 1980; Verbrugge, 1976, 1981, 1982; Waldron, 1976, 1982:103, 1983a, 1983b; Wiehl, 1938; Wingard, 1982, 1984 [Miller, 1981; Waldron, 1986:172]		Grubb, 1974; Goy & McEwen, 1980: 131; Hamilton, 1948; Hamilton & Mestler, 1969; Hazzard, 1984; Kendig & Hutton, 1979; Robinette, et al., 1957

* Exceptions and qualification are presented in brackets.

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(b) early sexual experiences [Higuchi, 1967; Jessor, *et al.*, 1983],
(c) sexual promiscuity [Farrington, 1982:190; Robins, 1966; Wilson & MacLean, 1974:853],
(d) child abandonment, neglect, and abuse [Alfaro, 1981; Bush, 1978; Glueck & Glueck, 1950:127; Kempe, *et al.*, 1962], and
(e) short life expectancy [Martin, et al., 1985; Robins, 1966:65].

Sex Differences in Victimful Criminality and r/K Selection

Over the past few years, several scientists have hypothesized that males should be more r-selected than females [Masters, 1983; Ellis, 1987, Rushton & Bogaert, 1988:270; Szacka, 1989:601]. If so, an aspect of modern evolutionary theory actually predicts greater victimizing tendencies among males than among females.

To see how various writers have deduces that males should be more r-selected than females, four points may be made. First, while the concept of r/K selection has been frequently used in recent years to explain between-species variations in traits, applying it to within-species variations is also common [Gadgil & Solbrig, 1972; Jolly, 1985:42]. In fact, as originally conceived, r/K selection was primarily employed to help explain intra-species variations in traits, not between-species variations [Menge, 1974:84].

Second, it is fundamental to the concept of r/K selection to assume that genetic factors are playing a contributing role [Taylor & Condra, 1980:1192]. Such an assumption is consistent with the fact that an entire set of genes serves to differentiate the sexes (many of which are located on the Y-chromosome), and that these genes have been evolutionarally stable across the entire mammalian order [Vogel & Motulsky, 1979:77].

Third, there is considerable evidence to support the view that sex hormones (whose levels are highly influenced by genes, especially those located on the Y-chromosome) are primarily responsible for sex differences in r/K selected traits

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[Ellis, 1989; Lynn, in press].

Fourth, from the standpoint of evolutionary theory, the primaril basis upon which it can be surmised that genes located on the Y-chromosome must be helping to promote r-selected traits comes from noting that only females gestate offspring. Thus, gestation is an initial form of parental investment that only females can make. Once this investment has been made, females are subsequently favored for continuing to invest in the offspring after birth (or hatching) to a greater degree than males [Hardy, 1981:22; Daly & Wilson, 1983:93]. This sex difference leaves males with the option of investing a greater proportion of their "reproductive time and energies" in other aspects of reproduction such as procuring additional sex partners. In other words, within nearly all populations, natural selection should have favored males who spent somewhat more time than females competing over copulatory access to multiple sex partners instead of caring for offspring.

According to the above reasoning, natural selection has favored the accumulation of genes on the Y-chromosome which incline males to be somewhat less prone than females toward caring for offspring (a basic form of altruism). Inasmuch as altruism (especially altruism with low probabilities of ever being reciprocated) is a significant correlate of K-selection [Gould, 1982:355], one would expect to find that, within species, females should be somewhat more K-selected than males.

The Hypothesis

The present inquiry was undertaken to determine if there was support for the hypothesis that, within mammalian species, males are more r-selected than females. The answer was sought by reviewing the literature on humans, on primates generally, and on other mammals, for evidence of sex differences in the above listed r-selected traits. If the hypothesis just outlined was correct, one should find that, within virtually all species, males will exhibit all, or nearly all, of the eight r-selected characteristics to a greater degree than do females.

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Evidence for Sex Differences in r/K Selection

To summarize the evidence pertaining to whether or not male mammals are more r-selected than females, Table 2 displays the results of compiling references to reports that have compares the sexes on various r/K selected traits. Exceptions are bracketed near the bottom of the appropriate cell. These exceptions either failed to find significant sex differences, or actually found females exhibiting a particular r-selected trait to a greater degree than males.

Numbers Conceived

Consistent with the hypothesis that males are more r-selected than females, studies have repeatedly found that more males are conceived than females. This is true for humans (15 reports), for nonhuman primates (2 reports), and for nonprimate mammals (3 reports). Three exceptional studies were located in the case of humans. These studies – all based upon fairly small samples – either failed to find significant sex differences among aborted fetuses or found higher proportion of females than males. Also, somewhat contrary to prediction, two studies in cattle which compared twinning and singleton births found twinning more common in the case of female calves than male calves.

Gestation Length, Miscarriages, and Neonatal Mortality

Fetuses that are born prematurely or in non-viable conditions should be indicative of r-selection. Studies of humans (5 reports), of nonhuman primates (1 report), and of nonprimate mammals (1 report) all indicated that males are more likely to be born in non-viable conditions than females. Nevertheless, contrary to prediction, in the case of one study of cattle who had been artificially inseminated (making the date of conception precisely known), males were found to have been born after a slightly longer period of gestation than females.

Maturation to Sexual Functioning

Among humans, females generally mature more rapidly

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than males in a wide variety of motor development skills [Hutt, 1972; Denckla, 1973, 1974], and even in the appearance of various secondary sex characteristics [Asayama, 1975; Dreyer, 1982:561; Irwin & Millstein, 1986:86; Kalat, 1988:304]. This more rapid rate of physical maturation for females than for males is true of nearly all mammals [Hines & Gorski, 1985:76], including primates other than humans [Nieuwenhuijen, Slob, & van der Werff ten Bosch, 1988:367]. While this may seem contrary to the hypothesis that males are more r-selected than females, one should keep in mind that the key sexual maturation variable for r/K selection is not general maturation or even the development of secondary sex characteristics. It is specifically the readiness to begin copulating and the ability to begin contributing to the conception of offspring.

As shown in Table 2, the hypothesis that male mammals are more disposed toward mating at an earlier average age than females is strongly supported, at least among primates. Specifically, males have been found to exhibit an earlier onset of sexual activity than females according to studies among humans (9 reports), and among nonhuman primates (5 reports). No studies of nonprimate mammals were located.

Fertility Potential

Evidence that males have a higher fertility potential than females has been noted for humans (6 reports) and for mammals generally (3 reports).

Copulatory Frequency or Strength of the Sex Drive

That males have, or at least seek to have, sex more frequently than females has been documented for humans (17 reports), for nonhuman primates (3 reports), and generally for mammals (2 reports). Being a possible exception, one study of humans conducted in Germany found no significant sex difference in copulatory frequency among a sample of unmarried high school students. Also, regarding studies of purely heterosexual intercourse, Phillis and Gromko [1985] recently noted that the involvement of males and females must be, by

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definition, equal, and that any account to the contrary should be viewed with suspicion.

Among the possible explanations for why males usually report greater numbers of heterosexual copulatory experiences than females is that (a) extremely active females (*e.g.*, prostitutes, "nymphomaniacs") are not fairly represented in colleges where most surveys are conducted, (b) the males who are sampled are often somewhat older than the females who are sampled, and (c) males may be more inclined to remember and even exaggerate the number of their heterosexual partners than females. Overall, at least in terms of motivation, the evidence supports the view that male mammals at least seek to copulate at higher frequencies than females, and humans throughout the world appear to be no exception.

Sexual Bonding

According to studies of humans (6 reports), males are less inclined than females to bond permanently to sex partners, especially to just one sex partner. Comparable data were not located for any other species, although the tendency for female harems forming around a single male in many species, and the almost complete absence of any male parallel to the forming of harems around a single female may be indicative of stronger bonding tendencies among females than among males.

Parental Investment

This variable was considered, first, with respect to the sex of the offspring, and, second, regarding the sex of the parent. Regarding the sex of the offspring, evidence pertaining to humans was mixed but largely supportive of the hypothesis. Nine studies found that human males were more likely to receive less (or lower quality of) parental care than females (as evidenced by rates of child abuse and abandonment), whereas 5 other studies essentially found the opposite (all of these exceptions came from Asian samples). Among nonhuman primates, all 4 reports indicated that males were more likely than females to be physically abused. However, in the only study located pertaining to a nonprimate mammalian species

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(mice), there was no greater tendency for males to have been abused than females (in this case the abuse under consideration was infanticide).

Concerning parental investment according to the sex of the parent, results clearly indicate that males invest less time and energy caring for offspring than females do – for humans (6 reports), for nonhuman primates (3 reports), and nonprimate mammals (9 reports). However, several studies of offspring abuse and infanticide, both in humans and in nonprimate mammals, indicate that females are more likely than males to injure or kill offspring – in humans (5 reports), and in nonprimate mammals (8 reports). These results may be explained without seriously damaging the r/K selection hypothesis simply by noting that they do not hold constant the time male and female parents tend to spend with infants [Erwin, 1983:81; Eagly, 1987:90]. Once time spent caring for offspring is controlled, it is rather certain that males would be more abusive toward offspring than females.

A few studies of rodents (listed as exceptions in the nonprimate mammalian column) reported that virgin females were just as infanticidal as males (regardless of breeding status), although females who had given birth to at least one litter were consistently less infanticidal than males. Among nonhuman primates, the one report listed as inconsistent with the hypothesis would be better considered a slight qualification rather than an exception. It found no sex difference in the attention and care given to infants by adults when one adult and one infant were along. However, whenever both sexes were in the vicinity of an infant, females nearly always took charge of the caregiving responsibilities.

Life Expectancy

Regarding life expectancy, no studies of nonhuman primates were located, However, studies of humans (43 reports) and of nonprimate mammals (7 reports) largely indicate that females outlive males in most populations by considerable margins. For humans, male life expectancy tends to be 85 to 95% of that of females. Only two exceptions were

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found for humans, and both were based on studies in southern Asia of relatively isolated populations where male longevity was reported to be slightly greater than for females.

Conclusions

This report extends proposals by Rushton [1985] and Ellis [1987] that criminal behavior (at least violent and property offenses) reflects an r-selected approach to reproduction. It contends that the main reason males are more prone toward serious criminal behavior than females, from an evolutionary standpoint, is that males are more r-selected than females.

The hypothesis that males are more r-selected than females was tested not simply at the human level, but by comparing the sexes of all mammals with respect to eight common indicators of r/K selection. This comparison yielded evidence largely supporting the hypothesis. Specifically, in most populations, relative to females, male tend to be conceived in greater numbers, are born after shorter gestation periods, are miscarried more often, and suffer higher neonatal mortality. In addition, compared to females, males begin sexual activity at an earlier average age (or at least attempt to do so), have higher fertility rates (at least for the males that sire offspring), seek to copulate with larger numbers of sex partners, and seem to avoid long term sexual bonding. Furthermore, compared to females, males are generally abused and abandoned more during infancy (except in the case of sexual abuse). As parents, males are more likely to abandon and abuse their offspring (at least after controlling for the time spend in proximity to offspring) than females. Finally, the male lifespan typically is shorter than the female lifespan throughout the mammalian order.

That nearly all of the above generalizations hold true as much for mammals in general as they do for humans adds credence to the view that significant biological/evolutionary forces underlie the sex differences examined. It is proposed here that the concept of r/K selection captures the essence of these forces at least as far as evolution is concerned, although more refined scientific concepts may eventually be devised.

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Building on evidence that r/K selection can account for human variation in criminality [Ellis, 1987], this report concludes that the concept also explains at least some of the sex variations in criminality. If so, socialization factors can no longer be considered entirely satisfactory for explaining why males are more prone toward crime than females. Finally, the hypothesis advanced on this article leads to a further testable prediction: In comparing different human populations, one should find positive correlations between sex differences in victimful crime rates and sex differences in most r\K selected traits.

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Editorial Note: Since going to press the author has advised us that due to last minute changes there are a number of additions to the aforegoing list of references. These will be published in the next issue of the Mankind Quarterly, Vol. 30, No. 3.

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F.J. Irsigler

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DETERMINANTS OF STATE PUBLIC WELFARE EXPENDITURES: AN ANALYTICAL SURVEY

Robert G. Mogull
California State University, Sacramento

There has been an abundance of research which has attempted to explain levels and fluctuations in jurisdictional welfare outlays. The purpose of this paper is to survey the accomplishments of those studies. A model is specified and primary socioeconomic and political factors are reviewed with respect to their explanatory association with public welfare spending at the level of states. The analysis presents the hypothesis and the findings.

Introduction

Since the close of the Second World War, public welfare expenditures by the aggregate of American states has ebbed but primarily flowed. States' total annual real welfare spending grew from $8.3 billion in 1946 to $60.4 billion in 1986, while as a proportion of total states' budgets it has increased from .113 to .182. An abundance of research has examined not only welfare outlays by states, but also the appropriations of the federal government and, to a much lesser degree, of local governments. Studies have attempted to explain spending both over time and cross-sectionally, with much of the empirical evidence contradictory. The purpose of this paper is to take stock of the accomplishments of these studies within a limited framework. A model is specified and selected socioeconomic and political factors are reviewed with respect to their explanatory association with public welfare spending – primarily at the level of states. The goal of this paper is to summarize the hypotheses and findings concerning those factors which presumably have the greatest influence on outlays. Further empirical work is not attempted here, but only a survey and an appraisal of previous research.

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Extent of Poverty

Income inequality creates social and economic stress and tension, which are ultimately transformed into political pressures upon governors and legislators. Although relatively ignored and powerless except during periods of widespread economic downturn, the poor achieved growing political influence – especially during the 1960s. This influence is presumably reflected by the responsiveness of state governments to spend for public welfare.

Piven/Cloward identify the years from 1965 to 1969 in particular as a period of explosion in AFDC cases (p. 187). The explosion in relief was due largely to the modernization of Southern agriculture and to its impact upon blacks. As blacks migrated to cities and experienced disproportionate rates of unemployment, social controls weakened and there were widespread outbreaks of disorder. Subsequently, the "growing mass of black poor in the cities emerged as a political force for the first time, both in the voting booths and in the streets. And the relief system was . . . one of the main local institutions to respond to that force . . ." (p. 196). "Economic convulsions" produced "mass turbulence" and electoral realignments, and the resulting political instability led to a liberalization of relief provisions (p. 197). Social workers and lawyers contributed greatly to stimulate the poor to apply for relief (p. 198) and to vote (pp. 222, 242). Political leaders were now "susceptible to a mass of new black voters" (p. 231). In addition, local governments were prodded by the federal government to liberalize its relief (p. 246). As a consequence, local relief agencies were confronted with an unprecedented volume of applications and pressures to grant benefits (pp. 249-50). In context of this analysis, these pressures would be reflected by a direct relationship between the incidence of poverty and state welfare budgets. The causal linkage is from unemployment (and thus poverty) to civil disorder and political activism, with subsequent economic concessions in the form of more public welfare.

The need for relief has been found to have a strong and direct empirical impact on welfare caseloads (Hicks/Swank, pp.

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709, 711). That is, apart from the linkage from poverty to civil and political activism, welfare policy is a function of the pool of eligible persons. This result, however, contradicts the Piven/Cloward contention that the pool of needy people does not automatically expand welfare rolls (pp. 189-90, 336-37). Hence, the magnitude of poverty may impact state public welfare outlays either directly and automatically or through the intermediary links of social and political activism.

Studies by Winters (p. 624) and by Plotnick/Winters (p. 471) confirm a direct relationship between needs and state redistribution efforts. Plotnick/ Winters suggest that the greater visibility of concentrations of poor people creates a more direct knowledge of their condition and affects the senses of the nonpoor (pp. 462, 471). This concept is implied in Orr's model of interdependent utility preferences. In his study of state AFDC assistance levels, "taxpayers derive utility from increments to the income of transfer recipients" (p. 359). Greater contact with the poor and observation of their distressed conditions motivate taxpayers to more willingly provide aid. Proximity to the poor would be communicated to the lawmakers or observed first-hand by the legislators (Plotnick/Winters, p. 466). On the other hand, perhaps a high incidence of poverty makes the nonpoor more aware of their own economic vulnerability and of the need for the insurance of an income safety net (Danziger/ Haveman/Plotnick, p. 70).

In contrast, after controlling for the Piven/Cloward riot effect, Jennings (1980) found an inverse association between social welfare expenditures and poverty. And, evidence from several models tested by Hirsch confirm an inverse relationship with poverty (pp. 87-90). Fry/Winters similarly found in a cross-sectional analysis that there is no correlation between state redistribution expenditures and a Gini index of income equality, and an inverse association with the proportion of families with less than $3,000 in annual income (p. 520). They explain this perverse outcome by concluding that the redistribution of state income is more a function of the participation by the poor than of their size (p. 521). Their conclusion

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conforms to the hypothesized linkage of from poverty to political activism to higher state benefits.

O'Neill echoes Murray in reversing the view that welfare spending responds to poverty needs. She argues that efforts to alleviate poverty through the tax and transfer system are partially offset by "changes in behavior induced by the promise of benefits" (p. 62). High welfare benefits encourage changes in family structure (pp. 67, 68; Murray, p. 152) and discourage work (p. 69; Murray, p. 150), thus increasing dependency in the long run (pp. 56, 69; Murray, p. 146). As to whether public welfare is responsible for causing poverty, O'Neill concludes that "the answer is complex . . . [but that] welfare may increase poverty indirectly if it leads to choices that close off options for self-improvement" (p. 72). Her examination finds that "high welfare benefit levels contribute to marital dissolution and the formation of female-headed families" (p. 72), a view also shared by Gordon (p. 341). To the degree that this is true, it is conjectural as to what degree transfer payments by states are determined by the poverty needs of residents, or that the function runs in the other direction.

From a political as well as socioeconomic perspective, poor voters "bargain" for additional transfer payments from a position of numerical strength. And taxpayers will acquiesce if there is some concomitant benefit to them. Presumably, such benefits would include the reduced likelihoods of crime, of social disturbances, and of uprisings which threaten their assets and position in the economic order. Thus, taxes designated for public relief represent insurance premiums to ensure the maintenance of domestic peace, security, and stability. Consequently, a direct association is again hypothesized between the extent of poverty and state public welfare budgets.

Incrementalism

"Budgeting is a very conservative process" (Dye, 1969, p. 462). Decision makers regard the previous year's budget as an expenditure base and generally focus upon only a narrow range of changes. They "talk of expanding their base by a certain percentage" (Davis/Dempster/Wildavsky, p. 530). And

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an examination of Congressional appropriations from 1947 to 1963 correspondingly indicates that federal budgetary decisions are linear and temporally stable. Thus, the current year's budget is determined primarily by last year's allocation with but minor and predictable percentage increments. Shocks and administrative changes are viewed as transient in an otherwise deterministic system.

Davis et al. admit that their models are not predictive, but are "backward looking" and explanatory. "[T]he budget process is only temporally stable for short periods . . . [and they] have no *a priori* theory to predict the occurrence of . . . changes" (p. 542). Nevertheless, the magnitudes of the estimated parameters in their regression models are affected by a variety of influences – including the political party in power, the personnel occupying high offices, and pressures from interest groups.

Wanat explains that marginal increments in the budget are circumscribed by the knowledge that revenues will not increase appreciably in the forthcoming year and that each governmental agency will receive its particular allocation based upon this expectation (p. 1227). Sharkansky found that acquisitive state agencies are restrained by governors and legislatures, particularly when state spending and debt are already at high levels (pp. 1229, 1231). According to Wanat, there is a "mandatory" component to the marginal increase in budget requests which is tied to exogenous economic conditions – such as inflation in operating costs, increased client population, and pay raises. These costs of "staying in business" rise regardless of any changes in the programs. He maintains that inexorable but small "mandatory" increases alone are adequate to explain the concept of incrementalism.

It would be a mistake, however, to infer that because current year appropriations are closely associated with the previous year's, that outlays are therefore self-generating and uncontrollable. Browning regards the term "uncontrollable" as a misnomer. All expenditures are ultimately controllable by a change in an authorizing law or by imposing or enforcing caps on entitlement programs. Further, "[i]f the increase in expenditures is the result of some previous policy decision, then the

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political determinant that is affecting expenditures is one which occurred in a previous time period" (p. 45).

Private Charitable Giving

Private giving by individuals enters into the theoretical framework in several ways. From one perspective, it is a measure of the willingness, generosity, and social conscience of the public – and governmental antipoverty efforts are critically dependent on sympathetic public opinion (Heclo). As an index of humanitarianism, charitable contributions reveal the public's desire to assist the less fortunate, but still must be transmitted through political representatives in order to affect appropriations. As Plotnick/Winters express the connection, "voter preferences do not influence the policy decision directly. Preferences only enter indirectly in a representative democracy via election of like-minded legislators and, after the election, by direct or indirect (via interest groups) communication to these lawmakers" (p. 464). Thus, the complementary relationship between private giving to charities (as a proxy for public compassion) and state welfare outlays is ultimately direct.

Interdependent utilities are presumed between the givers and receivers of private charitable contributions (Becker), and the motives for providing charity can be classified into two categories (Rodgers, pp. 180-89). Direct utility is obtained from the act of giving, where satisfaction is derived either from anonymously helping others or from the explicit approval of others. The actual impact of one's contributions on the well-being of recipients is irrelevant. On the other hand, ulterior satisfaction is obtained from the effect of one's contribution. There may be either a concern for alleviating the hardship of the poor or a desire to modify behavior. Investment in human capital would increase the marginal productivity of the poor for the mutual benefit of both givers and receivers. Successfully modifying the behavior of the disadvantaged will achieve the benefit of reduced taxes through lower expenditures both for public welfare and for law enforcement, as well as expanding the tax base.

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A second test involving private charitable contributions consists of a replacement effect, where the association with state welfare is inverse (Bird, 1970, p. 193). The implied assumption is that there is some limit on total charitable contributions and that there is an optimal mix of private and public efforts. An inverse empirical relationship between the contributions of individuals and the state would be harmonious with Tullock's argument. Rather than make a private contribution, "cognitive dissonance" is reduced by voting for a tax to aid the less fortunate which is shared by all taxpayers (pp. 26-29). In that way, the cost to the individual is diluted while the individual's conscience can be at rest. That is, it would "be wise to vote charitably and act selfishly" (p. 28).

Empirical evidence of a substantial "crowding out" of private charitable contributions by governmental transfer payments is found in studies by Abrams/ Schmitz (1984, 1978). Plotnick/Winters similarly obtain a statistically significant negative coefficient (pp. 468, 470). The inverse relationship between public and private transfers is a consequence of both a substitution and an income effect. Increasing governmental transfers has a substitution effect by lowering the social need for private charity and an income effect by reducing the disposable income of taxpayer contributors.

Likewise, it can be argued that individuals take into account public income redistributions when making intrafamily transfers (Becker, p. 4; Cox/DeFina). Private transfers among family members result from a utility maximizing allocation of assets, whereby ratios of marginal utilities to prices are equalized among competing family-generated services. The motivations for private income transfers can be either on the basis of love and concern for family members (Becker, p. 3) or on the basis of negotiating for and rewarding services performed within the family setting (Pollak), where services include companionship and adherence to family rules. In obtaining satisfaction from the transfer, a donor motivated by feelings of concern would be indifferent between directly contributing and allowing the government to act as an agent. If, however, transfers are motivated by an attempt to obtain

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services from the recipient, then substituting a governmental intermediary will erode the donor's bargaining leverage. Under this arrangement, in order to obtain equivalent satisfaction, the donor would be compelled to contribute more. As the government assumes greater responsibility for redistributing income, either private transfers are curtailed when motivated by concern (hence an inverse relationship with state welfare) or private transfers are stimulated if motivated by a desire to buy services (a direct relationship).

Sharing of Responsibility

Until the Great Depression, the provision of public welfare was the responsibility of local and state jurisdictions, while the federal government aided only needy veterans, Indians, and merchant seamen. In the Social Security Act of 1935, responsibility shifted to a shared obligation with the central government. Washington became authorized to provide matching funds to those states that assisted the needy aged, blind, disabled, and families with dependent children. Subsequently, the separation between federal and state responsibilities has virtually disappeared. In the words of Albritton, "welfare represents a policy arena characterized by a high degree of interaction among federal, state, and local governments . . . [with an] inextricable mix of influences and decision-making at all levels . . . . [And,] policy structures at the federal level have a profound impact on policy making among the states" (p. 378).

Sharing the obligation for public welfare lies not only between federal and state levels, but also with local jurisdictions. In many states, local governments still account for a significant portion of welfare funding – especially for Medicaid. And, the degree of shared responsibility has a significant impact on social welfare policies within a state. Hanson concludes that average Medicaid payments, for example, are strongly influenced by the share of program costs assumed by the state (p. 329).

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Political Parties

Hibbs (p. 66), Tufte (p. 83), and others find that the two major political parties offer sharply different policies. The Democratic party favors more of a redistribution of wealth and income than does the Republican party (Tufte, p. 74), and when the Democratic party gains political strength, it is used to expand social welfare programs (Lewis-Beck/Rice, pp. 16, 20, 26). "The Democratic party," according to Browning, "has traditionally favored a larger government role in providing for social welfare and economic intervention than has the Republican party" (p. 17). McClosky/Hoffmann/ O'Hara identify Democratic leaders as possessing a significantly stronger willingness than Republicans to employ legislation for the benefit of the poor, and the Democrats place a much greater emphasis on economic and social leveling (p. 420). Social welfare programs and tax policy are used by Democrats to redistribute income and to promote social equality and human welfare (pp. 416, 417).

The emphasis by Democrats on economic and social leveling is a product of the support the party receives from lower-income groups and from minorities (Congressional Quarterly, p. 1714; Dye, 1969, p. 99; McClosky et al., p. 420; Peltzman; *Times Mirror*) or from the "down-scale classes" (Hibbs) and the working class (Lewis-Beck/Rice, p. 15; McClosky et al., p. 420). McClosky et al. cite the strengthening of the egalitarian philosophy of the Democratic party as a consequence of the decline of left-wing politics after the Second World War and the subsequent transfer into the party of former and potential radicals (p. 420).

In sharp contrast, a large group of studies (Dawson/ Gray; Dawson/ Robinson; Dye, 1966, pp. 244, 245, 293; Fry/Winters; Gray; Hofferbert; Lewis-Beck; Plotnick/Winters; Winters) conclude that political parties have little influence on social welfare programs at the state level. In the words of Hofferbert, "the party system and its operation do not seem to go very far toward explaining the kind of policies produced in the states" (p. 82) and state welfare expenditures are unaffected by one party or the other (p. 81). Gray concludes that, over time,

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economic variables "have greater explanatory power and the direction of the relationships is more clear-cut than for the political variables" in state welfare policies (p. 250). Dawson/ Gray similarly find that socioeconomic factors predict more of the variance in state welfare efforts than do political factors (pp. 472, 474), and that politics within states is a comparatively weak variable and growing weaker (p. 471). Once economic development is controlled, Dye finds that the association between partisanship and state public welfare policy disappears (1966, pp. 142, 244, 253, 258, 259; 1969, p. 101). He also concludes that competition between the parties has no liberalizing effect either on state welfare policies (1966, p. 144) or upon tax and revenue levels (1966, p. 246).

The inconsistent empirical evidence may be due, in part, to the difficulty of identifying a single party position which applies uniformly across all states (Key, 1964, p. 284; 1949, p. 392). This is observed by various researchers. Hansen states that "the simple Republican-Democratic dichotomy . . . cannot begin to capture the diversity of state party systems" (p. 428). Since each party is divided into 50 individual state organizations, they lack a common national unifying ideology (Dye, 1969, pp. 93-4). The parties merely reflect the socioeconomic characteristics of local constituencies rather than a national philosophy (Dye, 1966, pp. 239-41, 247, 250, 293; Dye, 1969, pp. 108-09). Jennings strongly opinions that "party labels . . . are . . . meaningless" (1979, p. 429). Piven/ Cloward refer to Southern and Northern regional differences within the Democratic party as an "unmanageable" fragmentation (p. 252), with "[m]any Southern Democratic states . . . far more conservative than . . . Republican Northern states" (Hansen, p. 428). Dye suggests that the welfare policies of Democrats and Republicans in rural environments are more similar than the policies of a given party between rural and urban areas (1966, p. 141). Since the two parties within a state are competing in the same vote market, their policies have more in common with each other than with same-party policies in other states (Dye, 1969, p. 94).

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The lack of association found between the two-party dichotomy and state policies is also likely to be due to poor party discipline, particularly in states which lack effective party competition. Parties are more influential in those state legislatures when there is a high degree of competition (Dye, 1969, p. 137), and Carmines correspondingly identifies a direct and strong relationship between competition and welfare spending. Although the influence is "only effective where the parties represent separate and distinct socio-economic coalitions" (Dye, 1969, p. 138), the division of constituencies is really the basis for party influence in the state legislatures (Dye, 1969, p. 138). Consequently, the impact of party label upon policy is highly variable among the individual states.

Dye (1969) cites additional factors which further loosen the association between party label and policy – including the influence of interest groups, gubernatorial leadership, popular support for the governor, and interaction between the governor and the legislature (pp. 140-46). Although the governor is the leader of the state's party (p. 169) and his "control over the state budget is . . . his most formidable power" (p. 161), he has "little formal authority over members of his own party" (p. 149) or "to deal with the complex problems of his state" (p. 149). And state monies cannot be spent without legislative appropriation (p. 162). His real power depends upon his persuasive abilities (p. 149). Furthermore, many items of state spending remain outside of his immediate control (pp. 163-64).

Jennings (1979) points out that even though "lower-class support [may be] concentrated in one party, . . . welfare benefits can be expected to increase only if the party gains sufficient power to implement [the programs]" (p. 416). Further, the "generosity of a state's social welfare policies depends upon the organization and mobilization of the lower classes . . ." (p. 416). Opposition party control between the governor and the legislature or between the two houses can also create a barrier to the implementation of social welfare policies (p. 417). Hence, in contrast to the Downsian argument that divided control of a state government (as an indication of party competition) stimulates higher welfare benefits (p. 202),

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Jennings argues that a divided state government "frequently stands as a barrier to more generous welfare policies" (p. 417) – a conclusion also roughly shared by Dawson/Robinson (p. 289). Plotnick/Winters greatly broaden the approach to postulate that state redistribution policies are the result not simply of competition between the parties, but of its interaction with a host of economic and political factors (p. 471).

Fiscal Resources

Although riddled with contradictory evidence (see Albritton, p. 396; Fry/ Winters, p. 521; Gray, p. 250; Hanson, 1984, p. 326; Jennings, 1979, p. 424; Winters, p. 624), governmental spending has often been found to closely covary with economic activity. Most pertinently: Browning found expenditures for social welfare to increase with economic development (p. 168), Jennings identifies per capita income as responsible for a significant amount of the variance of state welfare spending (1980, p. 39), Orr shows AFDC payments to be "strongly influenced by state per capita income" (p. 368), Albritton concludes that "welfare recipients benefit from [the] spillover effects . . . [of the] general prosperity of non-welfare-related citizens" and that the economic well-being of a state provides the strongest link with welfare policy (p. 396), Plotnick/Winters find per capita income positively and significantly related to state welfare guarantees (p. 470), and Mogull concludes in separate studies that "the ability to assist the poor is, by far, the most powerful single determinant" of welfare appropriations (1981, p. 408) and that "fiscal resources are the prime determinants of state and local antipoverty expenditures" (1978, p. 296). Dye (1969) states that "All available evidence indicates that economic development is significantly related to . . . welfare policy in the states" for all major programs (p. 429) and that a "state's income is the single most important variable determining the level of welfare benefits" (pp. 429, 431).

Yet, to the degree that a relationship exists between transfer payments and the fiscal condition of a state, the association is not necessarily clear or simple. According to Plotnick/Winters, "the conflict between economic and political

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explanations of redistribution is an artificial one" (p. 471). Sharkansky/Hofferbert find interactions of state wealth with both high voter turnout and intense interparty competition responsible for high levels of welfare expenditures (1969, p. 878). Likewise, Browning emphasizes that "politics and economics interact to affect the level of expenditures" (p. 26), and Dye concludes that social and economic conditions together determine state budgets (1969, p. 164).

But, a close statistical relationship between the fiscal health of a jurisdiction and subsequent spending on welfare does not explain the bases for that association, and various explanations may be envisioned. Proxies for economic strength are indices of the ability of a jurisdiction to supply/ afford public services. Orr, for example, accounts for the supply function by noting that higher taxpayer incomes cause a shift down in the marginal cost of supplying income transfers (due to the diminishing marginal utility of higher income). And, intersection of the lower marginal cost curve with a given demand curve (the marginal benefit curve of taxpayers) occurs at a higher equilibrium level of transfers (p. 363). Plotnick/Winters express this somewhat differently by remarking that greater affluence allows the nonpoor to "indulge at higher levels their impulse to aid the needy" (p. 462). From still another perspective, Danziger/Haveman/ Plotnick reason that political tensions are eased for increased welfare spending as higher per capita incomes soften the financial burden (p. 77). Heclo echoes the political perspective by observing that "there is more tolerance for poor people's programs in periods of rising real incomes" (p. 337).

An alternative explanation of the association between economic activity and transfer payments is through demand, as originally advanced by Adolph Wagner in the late 1800s (pp. 1-8; Herber, 1979, pp. 294-99). Wagner theorized that aggregate governmental expenditures increase as a nation industrializes. Referred to as his "law of expanding state activity" (p. xviii), Wagner's "rule" formulated that state expenditures grow both in absolute terms and as a percentage of national income (pp. 7, 8). With technological advances and

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the breakdown of relative self-sufficiency, citizens come to rely more and more on the state to provide economic security (Bird, 1979, p. 84). Hence, the income-elasticity of state expenditures exceeds unity or is "elastic." Although the hypothesis was designed to describe a long-term association (Bird argues that it was a prescription for what Wagner believed "ought to happen" – 1970, p. 71) of social progress, it has nevertheless been used to "explain" the demand for governmental services.

Furthermore, since the income-elastic response of government expenditures is hypothesized to occur during an industrialization phase, it is doubtful that the post-WWII U.S. economy offers a suitable test. Indeed, Beck's study concludes that the United States is a mature post-industrialized nation. Since the public sector has declined as a proportion of national product in the U.S. from 1950 to 1970, either we have entered a post-industrial phase or Wagner's thesis is proven invalid. Other studies, on the other hand, do indicate a growing public sector in the U.S. since the close of the Second World War (Dubin; Lewis-Beck/Rice, pp. 3-6, 22-3, 26; Lowery/Berry, p. 686).

In any event, a long-term relationship between economic activity and government transfers is expected to be close, strong, and direct. The association may, however, conceal interactive causal relationships, and it remains theoretically unclear as to whether the functional association with public welfare is due to supply or to demand or to an equilibrium composite of both. These issues cause Bird to stress that the "income-elasticity of public expenditures . . . is singularly barren of fruitful hypotheses 'explaining' in any meaningful sense the growth of public expenditures" (1970, p. 96).

Grants-in-Aid

According to Hansen/Cooper, "Federal aid . . . is the best predictor of state expenditure growth" (p. 26), and Gramlich contends that "without federal matching [grants], . . . [state welfare] benefits would drop to . . . nothing" (1986, p. 347).

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Extrajurisdictional grants (a) reduce the relative price of aided services, (b) increase the income of the benefitting community, or (c) affect both income and relative prices (Gramlich, 1977, pp. 222-24). The effects are simulated by a shift down of the marginal cost curve facing taxpayers (Orr, p. 362). Of the three, type "a" has the greatest impact on the provision of public welfare, depending upon the price elasticity of demand (Gramlich, p. 225). Grants which affect community income (type "b") have their impact dissipated over a variety of expenditure categories and, therefore, have a smaller identifiable effect (p. 224). Type "c" grants, however, are most relevant to our discussion because they represent the typical case (p. 231).

Gramlich concludes from an extensive review of research that categorical welfare grants stimulate lower jurisdictional spending by an amount roughly equal to the grant (p. 234). (Goetz argues that states overcommit themselves to programs under the lure of federal aid by not recognizing the full and hidden costs of maintenance and operation – pp. 184-85.) Similarly, federal AFDC grants (as an open-ended matching federal commitment) are found by Orr to have no significant income effect on state AFDC transfer payments, since they displace state funds of equal dollar value. The marginal price effect is significant, however, and changes in the AFDC matching formula have a nonnegligible impact on state benefit levels (p. 366).

Since the 1950s, federal grants have been apportioned among states based upon populations, needs, fiscal resources, and payments to recipients. In early years, grants were provided in equal amounts to outlays by the individual states. Under post-1950 modifications, the central government pays a greater share of low state AFDC benefits. But beyond a prescribed limit, the federal government pays states a fixed proportion of AFDC payments per recipient (as well as a fixed proportion of expenditures under Medicaid – the fastest growing and most expensive welfare program). Consequently, states are rewarded for low recipient payments (Dye, 1969, p. 431; 1966, pp. 127, 140) and also are encouraged to provide

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benefits to larger numbers of people (Dye, 1966, p. 129; Albritton, pp. 378-79), thereby obtaining a greater portion of funds from Washington (Dawson/Gray, pp. 445, 456, 459-60). Thus, the federal grants formula has been successful in reducing disparity among the states, simultaneously, in welfare payments per recipient (Dye, 1966, p. 133), in budgetary shares going for welfare (Dawson/Gray, p. 461), and in total public assistance payments (Dawson/Gray, pp. 467, 470). Indeed, federal grants make the difference between solvency and bankruptcy in state budgets (Albritton, p. 378).

With the increasing federal participation in public welfare programs, grants to states grew especially rapidly during the 1960s. However, when Ronald Reagan assumed the presidency in January 1981, the previous cooperative relationship with subnational levels dramatically changed to a decentralized system. The Omnibus Budget Reconciliation Act of 1981 significantly reduced federal grants, consolidated diverse categorical grants into broad purpose block grants, loosened restrictions on the use of funds, and eliminated the matching feature of certain types of grants (Herber, 1985, p. 52; GAO, 1985, p. i). These changes caused a substantial contraction of state (and local) proportionate welfare spending for two reasons. First, block grants stimulate recipient jurisdictional spending to a smaller degree than do matching categorical grants (due to a reduction in the "price effect"). And secondly, the greater discretion in spending enjoyed by the lower levels permits a shifting of funds into alternative programs.

Tax Burden

Taxation is an "extraction of resources from citizens" and the question of who pays (and who benefits) is a political issue (Hansen, p. 416). The extraction is far more burdensome to the poor, since the proportion of income paid in state and local taxes gets progressively and strikingly smaller as incomes rise (Penniman, p. 534; Pechman/Okner, pp. 55, 60; Phares, pp. 218, 219; Pechman, pp 30, 60, 61). Yet, the bulk of the higher taxes which were necessary to pay for the War on Poverty and for programs of the Great Society were borne by

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the nonpoor (Danziger/Haveman/Plotnick, p. 69), and Buchanan argues that the principal opposition to extensions of state public welfare programs comes from the middle class (1967, p. 198).

Consequently, along with a constant rise over the years in the tax burden is a growth of tax resistance. This resistance took form during 1978 in the passage of California's Proposition 13, as a protest primarily against high property taxes (Urban Institute, pp. 15, 16; Citrin/Green, p. 15) and its perceived relationship to high welfare expenditures (Urban Institute, p. 17; Citrin/Green, p. 19). Danziger et al. confirm that the antipoverty policies of the War on Poverty and the Great Society created higher taxes (p. 69), which they estimate to have risen by between $115 billion and $195 billion in 1980 (pp. 71, 72).

Hansen finds, however, that revenue elasticity and legislated tax changes have little impact on state spending (p. 439). Although the relationship between revenues and subsequent expenditures was direct between 1960 and 1970, it turned negative between 1970 and 1976 (Hansen/Cooper, pp. 28, 32). [It is interesting to note that Hansen's model implies support for Bird's contention that revenues determine expenditures rather than the reverse, and that "[o]nce taxes are imposed, they . . . almost automatically give rise to future expenditure decisions" (Bird, 1979, p. 92). Buchanan, on the other hand, regards the direction as dependent upon "the particulars of each situation or conflict" (1967, p. 96).]

Clayton maintains that "[t]ax revenues are a good measure of public commitment to social purposes" (p. 48), and that resistance to taxes rather than to spending is the key indicator of support for welfare (p. 102). Similarly, Buchanan states that a considerable part of the demand by the taxpaying public for transfer payments is dependent upon their willingness to accept the tax burden (1977, p. 6). Correspondingly, Mogull employed a tax burden ratio to account for public welfare expenditures and found the association both significant and strong. Moreover, public welfare spending is becoming

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increasingly related to the burden of taxes (1981, pp. 407, 409; 1978, p. 299).

Rodgers postulates that individual taxpayers minimize the present value of taxes which are used to finance a poverty program (p. 188), since transfer payments represent an investment in human capital which both raises productivity and reduces undesirable social externalities. Taxpayers with the goal of human investment would seek a program which emphasizes human capital formation in order to raise the marginal productivity and earnings of the poor beyond the poverty threshold (p. 188). But, our knowledge is too limited to determine either the kinds or levels of transfer payments/ human capital investments necessary to minimize the present value of poverty tax payments (p. 188).

Lampman similarly recognizes the benefits accruing to taxpayers, but emphasizes that there is no way to determine when a level of transfers is efficient in the sense of attaining equality between marginal benefits and tax costs (p. 89). The issue is further complicated by externalities, where community benefits exceed the sum of benefits to individual taxpayers and the appropriate goal is to equate societal marginal benefits and costs (pp. 89, 90, 98, 101). Moreover, many benefits either are intangible or are difficult to quantify, thereby making it impossible to conclude when incremental benefits and costs are equal. Among the intangible costs are the reduced freedoms inherent in expanded social welfare activities by a paternalistic and coercive authority (Buchanan, 1967, pp. 99-101). An additional concern is the "fiscal illusionment" or camouflaging of true costs (intentional or not) and the subsequent distortion and inefficiency in the allocation of resources (Goetz; Buchanan, 1967). In short, taxpayers do not and, with our present state of knowledge, can not know the real costs and benefits of governmental activities such as public welfare.

State Debt

Since spending can be financed through public debt as well as through taxes, the method of payment may create false conceptions or fiscal illusions (Buchanan, 1967). By permitting

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a debt alternative to taxes, the link between costs and benefits becomes more difficult to construct (p. 104) since the two choices become independent in any current period sense (p. 100). Allowing for debt widens the divergence between a rational weighing of the costs and benefits in making a fiscal decision (p. 100) and "makes any reasoned comparison of benefits and costs almost impossible" (p. 112). In contrast to a no-debt model, the public perceives a lower value to the cost of making expenditures, becomes more accepting of larger public outlays, and is "more reluctant to levy current taxes" (pp. 100-01). Similarly, the strict requirement of a balanced budget during periods of unemployment also distorts the calculus of taxpayer decisions by overestimating the real costs of public expenditures (p. 111). Buchanan does not, however, draw the normative conclusion that budgetary deficits make public spending too large, but only that it "tends to be larger than and taxation less than they would be in the absence of the debt . . . alternative" (p. 104).

According to Sharkansky, the most hospitable environment for expanding public services occurs in those states where per capita expenditures and debt are low and per capita personal income is high (p. 1223). Yet, debt may be the only feasible method of immediately financing necessary social welfare programs for, as Hansen states the dilemma, "State officials often fear that espousal of a new tax or tax increase is tantamount to political suicide" (p. 419). This fear may go far in explaining the highly conservative process involved in the making of state budgets (Dye, 1969, p. 462) and the elevated responsibility of governors when deciding upon taxes and debt (p. 461). What is attributed to the incremental method of arriving at a new budget may be, in essence, the operational manifestation of that fear. Many states, however, also have either a statutory or a constitutional restriction on indebtedness, and the overwhelming majority of states have a balanced budget requirement. Moreover, among state officials there exists a balanced budget "mind-set" (GAO, 1988, p. 53). Consequently, the bulk of state revenues is necessarily derived from state taxes.

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In brief, the Buchanan and Hansen hypotheses predict a direct association between debt and public welfare appropriations while the Sharkansky analysis anticipates an inverse relationship.

Concluding Remarks

Buchanan regards the explosive growth of government at all levels as "alarming" and the future picture as "horrendous." Consequently, "the need to understand why government grows so rapidly seems urgent . . . . [If] the current spending explosion is to lead to effective political countermeasures, if the explosion is to be stopped or even slowed down, we must have some understanding, some explanation, of why it is occurring. We must explain the institutional and political processes that produce the results that we see . . ." (1977, p. 3).

This survey is a part of the continuing investigation into identifying the systematic components of public policy at the state level and, in particular, into finding laws of association to explain public welfare expenditures. Research of this nature was originally launched by Solomon Fabricant in his study of 1942 interstate expenditure variations, and has been extended by the efforts of many subsequent investigators. Fry/Winters regard the states as analytically distinct and unique jurisdictional entities (pp. 511-12) and Gramlich refers to the states as true social laboratories (1986, p. 225). The conceptual model which established the framework for this survey has been adopted and assimilated from an extensive list of contributors. The independent dimensions are presumed to account for the principal economic, social, and political characteristics associated with state public welfare appropriations.

Although many elements may influence behavior in political choice and the would-be scientist may feel himself lost (Buchanan, 1967, p. 170), we must nevertheless proceed on the presumption that "behavior in politics can be explained and predicted" and that "the validity of an economic model of . . . behavior . . . extends over wide areas of real-world choice" (p. 171). The task of the scientist is to explain how political decisions get made, how much money is spent in the public

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sector, and how costs and benefits are to be distributed. To these purposes, analytical and explanatory models of group behavior are constructed (p. 173).

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THE 'DEMOGRAPHIC PROBLEM' IN ISRAEL*

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The 'demographic problem' (see Nigem and Nagi 1988, for further details) is simply the following: the percentage of the population of Israel that is non-Jewish is growing (see Table 1; please note that the Jewish settlers in the West Bank and the Gaza Strip are all Israeli citizens and are therefore counted as Jews in Israel proper). But there has been some dispute about this; see Benvenisti (1984) and Teplow (1987) for the view that this proportion is not growing, and see Soffer (1988), Watson and Kubic (1988), and Friedman (1987) for the view that this proportion is growing. The purpose of this paper is simply to investigate this problem, using the official statistics of the Central Bureau of Statistics in Israel.

As can be seen from Table 1, the proportion of non-Jews of Greater Israel (pre-1967 Israel plus East Jerusalem, the Golan Heights, the West Bank, and the Gaza Strip) did not grow between 1967 and 1982. Since 1982, it has been growing at about 0.35 percentage points per year. Benvenisti and Teplow only consider the data up to 1982. The others consider the data after 1982. This is why Benvenisti does not find that the proportion of non-Jews in Greater Israel has been growing very much and why therefore it does not constitute a big problem for Israel. On the other hand, those holding that there is a demographic problem in Israel look at the data after 1982 and are worried. Now, which should we pay attention to? This is what I'd like to explore a bit in the next two paragraphs.

Why didn't the proportion of the non-Jews in Greater Israel grow between 1967 and 1982? There are three reasons: (1) there was net emigration from the occupied territories, due to the war in 1967 and the booming economies of the Arab world (see Benvenisti 1984 and Central Bureau of Statistics 1988); (2) there was net immigration of Jews into Israel in this

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Table 1
The Population of Jews and Non-Jews in Israel Proper and Greater Israel, 1967-1987.

Thousands, unless otherwise stated.

	Israel Proper[1]				Greater Israel[2]		
	Jews[3] (a)	Non-Jews[3] (b)	Total Population at end of year (c)=(a)+(b)	Non-Jews as % of total (d)=(b)x100/(c)	Non-Jews (e)	Total Population at end of year (f)=(e)+(a)	Non-Jews as % of total (g)=(e)x100/(f)
1967[4]	2,383.6	392.7	2,776.3	14.1	1,359.4	3,743.0	36.3
68	2,434.8	406.3	2,841.1	14.3	1,346.2	3,781.0	35.6
69	2,506.8	422.7	2,929.5	14.4	1,384.5	3,891.3	35.6
70	2,582.0	440.1	3,022.1	14.6	1,417.9	3,999.9	35.4
71	2,662.0	458.7	3,120.7	14.7	1,460.1	4,122.1	35.4
72	2,752.7	472.3	3,225.0	14.6	1,492.8	4,245.5	35.2
73	2,845.0	493.2	3,338.2	14.8	1,547.1	4,392.1	35.2
74	2,906.9	514.7	3,421.6	15.0	1,598.4	4,505.3	35.5
75	2,959.4	533.8	3,493.2	15.3	1,634.5	4,593.9	35.6

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76	3,020.4	555.0	3,575.4	15.5	1,675.7	4,696.1	35.7
77	3,077.3	575.9	3,653.2	15.8	1,722.4	4,799.7	35.9
78	3,141.2	596.4	3,737.6	16.0	1,767.4	4,908.6	36.0
79[5]	3,218.4	617.8	3,836.2	16.1	1,781.1	4,999.5	35.6
80	3,282.7	639.0	3,921.7	16.3	1,819.8	5,102.5	35.7
81	3,320.3	657.6	3,977.9	16.5	1,858.3	5,178.6	35.9
82[6]	3,373.2	690.4	4,063.6	17.0	1,917.0	5,290.2	36.2
83	3,412.5	706.1	4,118.6	17.1	1,972.4	5,384.9	36.6
84	3,471.7	728.0	4,199.7	17.3	2,031.3	5,503.0	36.9
85	3,517.2	749.0	4,266.2	17.6	2,091.5	5,608.7	37.3
86	3,561.4	769.9	4,331.3	17.8	2,152.6	5,714.0	37.7
87	3,612.9	793.6	4,406.5	18.0	2,217.7	5,830.6	38.0

Notes:
1. Pre-1967 borders plus East Jerusalem and the Golan Heights.
2. Israel Proper plus the West Bank and the Gaza Strip.
3. Includes all Israeli citizens in the occupied territories.
4. Israel Proper includes East Jerusalem.
5. The El-Arish population - about 30,000 - was deducted from the Gaza Strip after its return to Egypt.
6. Israel Proper includes the Golan Heights; the population of the Rafah Area - about 7,000 - was deducted from the Gaza Strip after its return to Egypt.

Source: Central Bureau of Statistics (1988), pp. (16), (99), 31, 705.

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period (see the same sources); (3) some non-Jews were returned to Egypt in that period (see Table 1). After 1982, all three of these reasons for the proportion not growing did not obtain. There was no longer net emigration from the occupied territories to the Arab states because of the collapse of oil prices (see Benvenisti 1984 and Soffer 1988), net immigration of Jews to Israel dropped (again see Benvenisti 1984 and Soffer 1988), and finally there was no more return of population to any of the Arab states.

Can we expect the situation after 1982 to continue for the next 20 or 30 years? Assuming there is no change in Israeli policy, then we cannot expect the return of non-Jewish population to any of the Arab states. Nor can we expect much net emigration out of the occupied territories to the other Arab states, particularly those on the Persian Gulf, because we cannot expect a boom in their economies. That leaves us with Jewish immigration. The Likud party obviously expects a renewal in net immigration of Jews to Israel in the future, particularly from the Soviet Union (see Friedman 1987). But it is not realistic to expect a big surge in net immigration of Jews to Israel in the future from any source. Finally, we cannot expect a big drop in the birth rate of the non-Jewish population in Greater Israel over the foreseeable future. Therefore, we should expect the proportion of non-Jews in Greater Israel to exceed 50 percent within 30 years or so. It is now growing between 0.3 and 0.4 percentage points per year (see Table 1).

In conclusion, there has been a debate about the existence of a demographic problem in Israel; and in this paper, we resolved this debate. No one knows, of course, the future. But at the same time there is no reason to expect the non-Jewish proportion of the population to remain the same over the future. This means that if Israel continues to occupy the West Bank and Gaza Strip, non-Jews will constitute a majority of the population within thirty years. They already constitute over 38 percent.

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CAN A MATHEMATICIAN BE A SOCIAL SCIENTIST?
The Case of William Shockley

J.W. Jamieson
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There are few more emotional questions than eugenics, or what Professor William Shockley of Stanford University – more appropriately perhaps – termed "dysgenics." Years after the Nobel Prize winner's attempt to persuade the National Academy of Science to set up a panel of pre-eminent scholars to study the heritability of IQ and of current intergenerational trends in IQ amongst the various ethnic stocks that comprise American society, the discussion of these issues remains distorted and generally suppressed by a multitude of political, historical, and cultural attitudes not necessarily linked to the sciences. From the Marxist far left – the ideological vantage point of his sharpest critics – Professor Shockley's efforts to awaken public opinion to the significance of genetic factors in determining the level of intelligence in future generations of humankind propelled a vicious, ongoing assault.

Because of his scientific stature, the only way that Shockley's critics could assail him effectively in the eyes of the public was to argue that his early background was in mathematics and physics, and that he was not qualified to comment on topics that fell within the social sciences. They consequently charged him with having an inadequate background in the traditional teachings of anthropology and psychology, and alleged that the years which he devoted to studying the relationship between intelligence, heredity and race, after his successes in the world of physics, were driven by bias, and that his perceptions were based on personal prejudice.

In light of these charges it may be of interest to look at the life history of this famous scientist, his opinions and achievements, and at the violent reaction, sometimes involving the intentional and violent disruption of his lectures, by those who made it a political duty to oppose him.

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Indeed, Professor Shockley was a genius who was bold enough to attempt to take the modern world from the "micro" realm of transistors, with their intricate latticework of man-directed energy, to the "macro" world of public policy issues, where decisions are often based on very emotional and very unscientific conceptions. His thesis, which enraged the radical political Left, was simple: Intelligence is a highly variable quality of prime importance to humankind in its struggle to survive, and even to the ability of the individual to be self-supporting. Statistical evidence indicates that intelligence, amongst other qualities, is largely determined by heredity. Further evidence suggests that contemporary Western society faces dysgenic trends, in that the less intelligent people were over-replacing themselves while the more intelligent were tending to under-replace themselves. Poverty and misery in future generations, and the future wellbeing of mankind as a whole could thus be better served if society would commission a detailed investigation of the entire question of heredity, intelligence and demographic trends in procreation and intelligence, so as to arrive at a broadly acceptable consensus regarding the facts. Furthermore, since it seemed patently obvious from the existing data that eugenic measures could affect the level of intelligence in future generations, Shockley also proposed a system of financial benefits to encourage individuals to participate voluntarily in eugenic programs.

William Shockley was a rare example of distinction and dedication. Considering the barrage levelled against him during much of the 1960s and 70s, he stood proudly above the contemptuous canards and fallacious allegations heaped upon him. Most observers familiar with his work contend that he pursued his scientific inquiries in a dignified way throughout the protracted ordeal, never allowing public calumny to discourage him from his perceived duty to draw the attention of the public to the debilitating effect of contemporary dysgenic trends. A man of strong personality and character, proud of his descent from bold, pioneering New England whalers, Professor Shockley was capable of withstanding the harshest forms of denigration and insult. Irrational and

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poorly-reasoned broadsides were levelled against him by ideologues in both the media and the academic world. A letter by one such academic, published in the American Anthropological Association newsletter of February 1970, even urged the "destruction" of such of his writings as might be found in academic as well as public libraries!

By putting Shockley on the defensive, much of his time was taken simply attempting to neutralize baseless charges of racism, fascism, and the supposed parallels to National Socialist philosophy allegedly inherent in his works.

Professor Shockley lived and wrote in trying times. He developed his interest in the intergenerational preservation of human intellectual abilities when the civil rights movement was at its height and, with, as its aftermath, the Black Power saga. Clearly, America — and the American conscience — was troubled by a legacy of injustice. The nation had scarcely come to grapple with the status of black Americans in the early 1960s, when it witnessed the horrors of a presidential assassination, and was then forced to endure an endless foreign war. What one contemporary writer, Julian M. Weiss, insists were "multiple wounds inflicted on the American psyche" during this period, perhaps made rational consideration of emotionally-charged issues all but hopeless. "In the climate of urban violence, a declining national spirit, ... and growing economic malaise" described by Weiss, Shockley's views were easy prey in the eyes of the well-organized Left.

For the Left Wing, stakes were high indeed. Shockley's views constituted a challenge to the ultra-egalitarian tenets which they had espoused for generations, and his status as a Nobel prize-winner and benefactor of mankind promised to orient the public to listen sympathetically to what he had to say. As will be documented, there is a direct conflict between Marxist pragmatism and Darwinian evolutionary science, which is far more profound than the theoretics of the dispute between capitalism and communism. But ironically, while the significance of the erroneous "nature-vs.-nurture" and "environment vs. heredity" controversy (erroneous in that no real opposition between heredity and environment as personality-

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forming factors really exists) is well known to avowed Marxists, who readily perceive the implications of this issue within context of the political struggle, but the importance of a true perception of the role of heredity in determining human potential is only poorly appreciated by the general public, who consequently fall ready prey to Leftist interpretations of history.[1]

While only a few observers of politics and world affairs have noted the significance of the Shockley-inspired debate, the extreme Left is fully cognizant of this challenge to their world view. To them, the gauntlet was dropped. The Nobel-prizewinning physicist and mathematical genius had to be silenced or at least discredited. Since he supported his views by sophisticated mathematical and statistical argumentation which the public could not be expected to follow, the answer was to intertwine the entire multifaceted scope of Shockley's theses in a single set of anti-intellectual cliches. The 'Father of the Transistor' perceived connections between inter-generational socio-economic conditions, the nature of intelligence, and the threat of declining intelligence evidence in the society as a whole, and diverse relevant arenas. His opponents sought to portray him as a misguided bigot, who was out of his element when discussing matters of race and heredity.

Given decades of politically leftist infiltration in the social sciences – decades that saw open Marxists receive plentiful foundation grants and secure positions of prestige at major universities – it was relatively easy for them to mount a concerted, orchestrated attack on anyone who opposed their still-unproven ideas concerning the roots of intellectual performance. "Marxist influence in the contemporary social sciences," declares J.W. Jamieson, "is perhaps most evident in the persistent attempts . . . . to keep alive the meaningless 'nature vs. nurture' controversy."[2]

There will be much to learn about the broad issues at

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1. Conway Zirkle, *Evolution, Marxian Biology, and the Social Scene*, Philadelphia: University of Pennsylvania Press, 1959.
2. *Ibid.*

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hand. Shockley's greatest contribution, in time, may well be his move to unleash the fullest forces of scientific inquiry into the extent to which genetic factors affect socio-economic conditions, as well as the reverse. As he stated on many occasions in letters to friends, and in whatever public lectures were not canceled for fear of violence or other pressures, it was his hope that the "best solution" to real obstacles encountered by lower income groups, whether black, white, or of any other ethnic background, would be revealed by scholarly and unbiased research into the relationship between heredity and ability.[3] That solution would help the impoverished segments of all races, he was certain, either by confirming the need for greater social action to improve their condition, or by laying to rest those policies which appear to "permanently enslave" the poor by fostering the process of dysgenics.

It is worth noting at the outset that his dispassionate discussions of dysgenic heredity — and in particular of the traits which demonstrably lower intellectual levels within groups of individuals — was never limited to any one racial group. Shockley forever placed whites along with other racial stocks in the same "control group" which was to have formed the kernel of his recommended research.[4]

Contrary to the image that has been conveyed to the public by the media, the validity of Shockley's assertions are not challenged by true experts. This is a dramatic contrast to the impressions created by sensational media accounts and by the unceasing attacks from the Left wing establishment both within and without academia. From a scientific standpoint, his basic assumptions are widely respected. For example, Mark Snyderman and Stanley Rothman are two hard-hitting scholars of standing. Rothman held a Ph.D. in psychology from Harvard, while Snyderman was a professor at Smith College. Rothman had already opened many minds in the 1970s with his landmark — and in-depth — studies on bias in the print and broadcast media. In 1988, their book, *The IQ Controversy: The*

3. WAS personal papers (1967).
4. *Ibid*, 1970.

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Media and Public Policy, was published by Transaction Books. Part of this volume deals with a detailed survey responded to by 661 men and women active in pertinent scientific fields. An overwhelming 94% of these respondents indicated that a belief in the Shockley hypothesis, namely that genetic factors play a major role in determining human personality and intelligence. It therefore directly influences such conditions as educational attainment, career advancement, and abilities in many spheres of human life.

The impact of genes in the development of personality traits was endorsed by nearly five out of six of the respondents. Inheritability of intelligence, and other cornerstones of the Shockley view were given strong support by a large majority the respondents in the Snyderman- Rothman probe.

The resultant Snyderman-Rothman publication exposes the way in which far too many members of the news media treated the whole subject. One characteristic of this bias has been the unsubstantiated charges, raised and executed in Pavlovian fashion, against all and any who question near- total environmentalist theories of human personality and behavior. Nearly a quarter of a century after the anti-Shockley treatises were first unveiled, the effect of the smear campaign is still evident. In his own book review of *The IQ Controversy*, Daniel Seligman, writing in *Commentary*, asserts that scholars who have advanced the case for substantial genetic control over personality "have been smeared as racists."[5] Ironically, one source for many journalists is a non-geneticist Marxist academic, Leon Kamin of Princeton, whose views on the subject are shared by only the smallest minority of scholars, but who nonetheless is frequently quoted in major media reportage on the issue. Kamin, writes Seligman, is "a Marxist who views IQ as an instrument of class oppression . . . "[6]

There are other points to bear in mind when considering the anti-Shockley diatribes of the past two decades. One is that disfunction throughout the inner cities of America in the 1980s

5. *Commentary*, Feb. 18, 1988, p. 26.
6. *Ibid.*

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wrought social dislocation upon the nation as a whole. Its most immediate victims were, of course, blacks themselves. Avoiding discussion of dysgenics and accepting blindly the extreme forms of environmentalism espoused by those who wish to blame poverty upon the capitalist system, ultimately traps Afro-Americans in the never-ending syndromes in which too many still suffer today. Also, despite considerable levels of social spending, solutions proposed by the anti-hereditarian social policymakers in Washington D.C. and elsewhere have failed to markedly resolve this situation.

While a large percentage of minorities have indeed "made it," it is the underclass – those lower I.Q. individuals whom Dr. Shockley correctly identified years ago as inassimilable and incapable of rising in the social strata – which appears to be in the same dire straits as in pre-1954 America. (1954 is selected because of the watershed Brown Supreme Court decision in that year.) Consider: Washington, D.C. spends far more per capita on public education than any of the 50 states, yet has the poorest track record by any and every criteria. Consider: although budget cuts were publicized in the Reagan era, the share of social spending as opposed to military spending was reduced only by a single percentage point by fiscal 1983, at which point social spending constituted 54% of all federal outlays. In dollar amounts, of course, many programs soared during the supposedly frugal Reaganite years. These points are only raised in order to demonstrate that the emphasis on environmental solutions are but part of the remedy for the tragedy of extreme poverty, and that over-emphasis on environmental solutions can never solve the problem in its entirety.

The spending of ever-larger amounts of money is hailed as a panacea; false illusions and high expectations are created; the perpetuation of poverty and misery continue. The late 1980s phenomenon of hard narcotics usage in the inner cities reveals how dependency on anti-hereditarian philosophies has damaged entire generations of minority Americans.

This would not be surprising to Shockley, who familiarized himself with the research conducted by environmentalists and

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hereditarians alike. But during the 1960s, the drive by committed Marxists accelerated. Its main goals were to obfuscate the findings of uncommitted scientists, while using the well-worn "fascist" tag as a canard to eradicate opposition within academia and other policy-influencing circles.

So long as the image of Man as totally dependent on environmental factors could hold sway, Marxist theories could continue to influence public policy-making. The Marxists in consequence found themselves obliged to sidestep scientific issues and substitute ideology for empirical evidence. "At present, 'sociological biology' has almost nothing in common with biology of the biologists," noted Conway Zirkle in *Evolution, Marxian Biology, and the Social Scene*. In fact, many examinations of the nature of intelligence, and of the debate surrounding IQ tests, were under way long before the Nobel prize winner publicly raised these issues:

"There has gradually been a shift of interest from concern with demonstrating the usefulness and scientific value of intelligence tests to the clinical value and significance of the test findings. In other words, current-day clinical psychologists want to know how well the person's performance in the standardized intelligence testing situation reveals the many aspects of his individual personality, organization, and functioning."[7]

During recent decades a large, and ever-increasing, proportion of Western treasuries has been devoted to combatting social problems. These are attendant to minority group performance in education, employment, and in other fields. What some refer to as a "social-industrial welfare complex" employs large numbers of people who have a great deal to lose should basic assumptions concerning the validity of those expenditures be challenged. Yet, as we shall see, it was never Shockley's intentions to dismantle the welfare state, but only to reduce, so far as humanely possible, the burden of welfare costs by eugenic methods – by seeking to decrease, or least reduce the rate of increase – the proportion of unemployables

7. Conway Zirkle, *Evolution, Marxian Biology, and the Social Scene*, Philadelphia: University of Pennsylvania Press, 1959.

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in succeeding generations. Nor were his attitudes shaped by any callous disrespect for minority achievements. Politically motivated detractors operating in a politically charged situation were responsible for creating this highly erroneous misconception. As Vermont Royster declared in 1968, there are "emotionally explosive" consequences inherent in discussion of the intelligence and genetic quandary."[8]

Proof that hereditary considerations predominate would topple Leftist assumptions. It would undercut the *raison d'être* for the statist policies continually advocated by well-meaning liberals. "For if it is true," wrote Royster in *The Wall Street Journal*, "it means that a large part of our public programs for welfare and for lifting up the disadvantaged are misdirected and possibly futile or even self-defeating, and that the whole national effort needs to be restudied and redirected."

As for the impact of Shockley on this debate, "an additional explosive ingredient" concerned "genetic deterioration" which was "more widespread in the lower strata of the Negro population."[9]

Shockley, as stated earlier, by no means concentrated on the genetic handicaps suffered by lower-achieving segments of the American minorities. He also turned his attention to similar problems amongst the whites. He frequently used both anecdotal as well as statistical information to prove that there was a strata of U.S. whites who can be clearly identified as intellectually handicapped, and whose performance in the standard criteria — education, achievement, skills, etc. — is dismal. Yet his academic critics and most segments of the media preferred to ignore this. Instead, they selected his references to black and minority problems, ignored the by ample and unbiased documentation with which he backed all his statements, and on the basis of this selective reporting "raised the dreaded word 'racism,'" as Royster puts it. "It seems to have done Shockley little good to assert his belief that (in regard to intelligence) 'many American Negroes are superior to many whites,' or to cite statistical studies showing that

8. *Intelligence: A Dynamic Approach*, Erika Fromm (Doubleday, 155, p. 44).
9. *Wall Street Journal*, May 22, 1968, p. 16.

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'Negroes achieve almost every eminent distinction that whites achieve.'" As Royster noted, the Shockley thesis was that "genetic deterioration occurs for whites as well as blacks."[10] But Marxists and fellow-thinkers preferred to ignore such statements, since their intention was to denigrate their author, to portray him as a biased authority, and thus to warn other scholars not to emulate his research.

It was against this background that the propaganda war against the California scholar continued for nearly two decades. By silencing Shockley, the Left could securely move on with its own agenda, one calling for a complete environmentalist approach. Sir Andrew Huxley, himself a Nobel Prize winner, defended Shockley's right to be heard. "Attempts to subordinate scientific judgement to political ends are misguided, even from a strictly practical point of view." Huxley was one who pointed to the "Emperors's Clothes" situation, as the entire focus of domestic spending programs was geared to solving problems based upon the environmentalist view of human nature and free will. "Policies based on untrue assumptions," he cautioned, "are likely to lead sooner or later to disaster."[11]

The Left stridently charged that the scientists backing Shockley were also hardened racialists. This is certainly not true at all, nor was there anything in Shockley's writings, pronouncements, and actions that ever suggested he was less than fully humanitarian. His personal papers reveal a concern prompted by the very idealistic sentiments which led liberals and anti-hereditarians to sponsor the wide variety of environmentalist-based programs which have produced so few results.

Another benefit to the Far Left in their anti-Shockley campaign was to use charges of "prejudice" as an effective rallying point – as this calumny had so often proved to be in the past. By making an "example" of a famous and honored figure who dared to speak out, the Left could through fear of meting out similar treatment prevent future possible oppo-

10. *Ibid.*
11. *Palo Alto Times*, Sept. 1, 1977, p. 4

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nents from speaking out lest they, too, be smeared as "racists." The negative image and heavy publicity given to small groups such as the Ku Klux Klan also played into Leftist hands. If the campaign was intended to make unsuspecting blacks and white liberals the pawns of communist and fellow-thinking ideologues, it succeeded well.

More potential cannon fodder for the Far Left was mobilized during the chaotic, confusing, and wrenching years when the twin swords of the extremist, anti-Western faction of the radical black movement combined with the pro-Hanoi forces that led much of the "anti-war" and "peace" movements flourishing at the time.

Dysgenics is certain to be given greater consideration as the social problems evident in the 1960s continue more or less unabated today – and seemingly threaten to expand to alarming proportions. The assault on Shockley, the silencing of his views, and the effective cover-ups which ensued, cannot possibly prevent the eventual release of material which, because of the *de facto* censorship campaign, never became truly public.

Shockley's opinions as described below should be seen as part of the search for answers – scientific and not ideologically-based – to a plethora of sensitive social maladies. He reminded audiences that crime rates in Denmark were some 2% per capita of those witnessed in Washington, D.C. (Imagine the comparison today, with Washington crime rates soaring to a frightening level, and with an average murder rate exceeding one per day). Denmark was a useful comparison, since that country had for some decades banned procreation by those deemed to be of extremely low intelligence.

In the post-Shockley era, another country is taking steps to secure sensible economic growth for itself and its citizenry. Economic reasons are generally credited with Singaporean official "encouragement" of citizens of higher IQ citizens to intermarry, raise families, and bear at least two children if possible. This idea comes from the proposals of that city-state's

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prime minister, Lee Kuan Yew, in 1987.[12] In *The Asian Century*, Julian Weiss reveals that this little known quest stems from non-racialist concerns. "A guided form of . . . Confucianist capitalism blended with a benign paternalism" has led to serious concern over the maintenance of the high intelligence usually associated with Chinese populations. "With but 2.6 million people, and endemic labor shortages projected . . . the increasingly-affluent city-state feels justified in promoting the most suitable characteristics in its populace . . . "[13] That country's "technology-oriented future depends on sheer brain power as a means of countering a lack of indigenous R&D and value-added activity such as chip design (as opposed to, for example, low-value semiconductor assembly)."[14]

Weiss reported that the government planned to promote social clubs for the exclusive purpose of having single men and women with high IQs meet – and, hopefully, marry. Most qualifying would be ethnic Chinese (74% of the population) and Indian immigrants (less than a tenth of the citizenry). Financial incentives, not too dissimilar from those proposed by Shockley, are also currently under consideration to stave off financial disaster by encouraging a more adequate level of reproduction amongst the more intelligent Singaporeans.

Ironically, the book also contains reference to neighboring Malaysia's plan to greatly expand its population from 16 million to a level exceeding 50 millions. With a small ethnic Chinese group clustered within their borders, the racially distinct Malays have seen a decade in which a nationwide *bumiputra* ("sons of the soil") plan, similar in many respects to affirmative action programs in the United States, sought to redistribute wealth. It is no secret that, for a variety of reasons, the Malays have not achieved socio-economic gains which were scored by the minority of Chinese, long active in commerce and trade. Their reliance on methods of improving education and their tendency to think in terms of demographic quantity

12. *The Asian Century*, (Oxford U. Press/Facts on File), p. 166-167.
13. *Ibid.*
14. *Ibid.*, p. 170

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rather than quality contrasts with that of the members of the more successful Chinese community living amongst them.

Media Attacks

As the debates over dysgenics and "nature-vs.-nurture" intensified, it came as little surprise that Dr. Shockley was hounded in much of the popular press. Of course, reporters would be able to offer grounds for criticism of the scientist's work if they rationally concluded it was inaccurate. The same applies if there were serious flaws in the methodologies used to support the general thesis.

Lingering questions, however, concern the accuracy of the media's reporting of Shockley's research findings and opinions, media accuracy in reporting the reactions of the scientific community to those statement, and media integrity in failing to report the self-acknowledged Marxist views of those academic critics who were most commonly cited as "authorities" in opposition to Shockley. As suggested earlier, the attacks that ensued during the late 1960s and throughout the 1970s cannot be understood outside the context of what was taking place in society. Another key point is that while the nation underwent its agonizing quest for social justice and the attainment of civil rights for blacks, any allegation of "racism" was certain to doom objective discussion of Shockley's views.

IQ tests were already controversial. They were challenged by both sincere critics as well as by those seeking to avoid fundamental questions about the role of heredity in human intelligence. Those tests posed disturbing questions for much of the Liberal as well as the Far Left ideology. The most sensitive issue, as far as political groups on the Left were affected, was the challenge posed to those who advocated increased social spending. If spending was not likely to achieve the desired results – or if only limited "environmental" results were possible – the virtue of massive increases in social programs would draw far less support from all levels of society.

One other point concerning media coverage is the question of whether the press ever fully understood the issue. Certainly, no probing analysis of the Shockley view ever took place in

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most of the negative commentary. His positions on public policy often coincided with those of the liberals on issues such as social programs. His staunch denial of absolute genetic determinism to the exclusion of all environmental influences (a patently absurd viewpoint which his opponents constantly sought to attribute to him), and his advocacy of financial rewards for those with defective heredity who voluntarily requested sterilization, were points disclosed only in media that afforded him balanced (and hence positive) coverage.

Lacking any clear understanding of genetics, of the nature and purpose of intelligence tests, and related topics, most reporters were ill-equipped to present a balanced view of Shockley's findings and recommendations. Furthermore, their reliance on a handful of decidedly anti-Shockley observers as the foundation for judgments on the subject of genetics and dysgenics ensured that media reportage of Shockley's position would be generally negative.

While not all the inaccurate coverage was intentionally so, the collusion of media mischief plus lack of scientific knowledge itself caused reporting to suffer. The importance of dysgenic trends, particularly to the minorities in America, was left out of most discussions. That factor alone, as future generations will doubtless concur, did a great disservice to society as a whole, and tremendous damage to the interests of the American minorities where, more than amongst the majority, the birthrate can be shown to be much higher among the welfare segment rather than the professional higher-income strata.

The harsh media coverage given to Professor Shockley included the crime of omission and biased selectivity of reporting. For example, when *The Detroit News* carried its *UPI* syndicated story – "Why Does Prof. Shockley Think Blacks Are Inferior?", it left out a portion of the original interview, and concluded its story with the totally ridiculous comment that: "If he is correct, the eugenic consequences are frightening. Unfortunately, we may never know the answer unless Prof.

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Shockley and his colleagues are liberated from prejudice."[15]

The tone of many accounts suggested that Shockley pitted races against each other. Of course, the scientific basis for what Shockley called "raceology" and dysgenics are far more profound than that. And, of course, Shockley readily pointed out "inferiority" when he found it among whites. Science knows no racial prejudice. Whites were included alongside blacks in Shockley's proposals for financial incentives to discourage by strictly voluntary means the procreation of those of extremely low intelligence and conversely to encourage the procreation of the more gifted whom he believed to be currently in danger of failing to perpetuate their numbers, under current socio-economic conditions, from generation to generation. The *New York Times* was among those which, while editorially supporting the cause of free speech and Shockley's right to appear as an invited guest speaker on campuses despite Marxist disruption, inaccurately proclaimed that one series of Shockley's lectures were "entitled 'on the superiority of the white race.'"[16] Needless to say, misrepresentation in such an important media vehicle — and through the distribution channels afforded by its own news services — did the Californian a tremendous disservice.

Throughout this period, the press ignored a central theme evident in Shockley's own writings, and in his actual public utterances. This was his "concern for the well-being of disadvantaged minorities" and hence, his "insistence on the moral aspects of the obligation to diagnose." By omitting this issue from the coverage of his views, and of the disturbances which were so often organized by the extreme Left to negate his public appearances, the press in effect censored — and worse still, frequently distorted — the presentation of his opinions to the public.

Furthermore, reporters were too often far off the mark when it came to researching the issues he discussed. Had many

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15. *The Detroit News*, "How and Why He Developed That Controversial Theory," Sept. 15 1974.
16. WAS papers, Jan. 23 1978, p. 3.

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contacted leading authorities on genetics, IQ performance and testing, and similar criteria, they would have fairly and objectively constructed the arguments held by the scientific community within the confines of academia, where theories are rigorously defined and debated. "Jensen's view is widely attacked by biologists and geneticists,'" erroneously commented *The Washington Post* in 1969.[17] "Shockley's own views were attacked by the Stanford genetics faculty two years ago as 'pseudo-scientific justification for class and race prejudice.'"[18]

Apart from the many biased accounts of Shockley's lectures and activities which appeared in the media, their treatment of incidents involving his attempt to persuade the National Academy of Sciences (NAS) to recognize the importance of research into group differences in heredity, which we shall shortly discuss, was distorted in many inaccurate press accounts. In its article, "Handler denies quotes in Academy Story," the now-defunct *Washington Daily News* (for years, and until its demise, the second largest daily paper in the U.S. capital) observed that NAS president Philip Handler had denied the accuracy of statements that had been attributed to him by the media, which had been made to appear strongly critical of Shockley.[19] Referring in this case to a Scripps-Howard syndicated feature article, Handler implored readers that "simplistic notions. . . are very dangerous. . ." and that "there are genetic influences but how much is genetic and how much is environmental" is "an extraordinarily difficult question."[20]

Astonishingly, Handler was misquoted in a way which completely distorted his own interpretation of Shockley's statements, and which severely twisted Shockley's own perceptions. Handler had been quoted as saying that:

"Shockley denies the environment a person is raised in is important to IQ and claims it's all genetics. I say it's exactly the

17. *The Washington Post*, Apr. 30, 1969.
18. *Ibid.*
19. *The Washington Daily News*, "Handler Denies Quotes in Story," June 16, 1971
20. *Ibid.*

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other way around."[21]

Attempts to clarify these types of distortions, which were representative of the unfair reporting surfacing over the years, were not well received by the print media. In many instances Shockley asked that a few words be included in news articles in order to clarify his position. He was usually denied that request. In time, he became so frustrated about unfair coverage that he was obliged to insist on keeping a tape recorder on hand during press interviews. Shockley resorted, at one point in 1978, to sending by registered mail letters and copies of interviews as a means of ensuring that his simple request to print facts instead of the distortions which appeared in print could not be overlooked except deliberately. Fighting the web of falsehood built up by the media to misguide the public as to his true position became an increasingly time-consuming process for this genius who dedicated so many years of his life to attempting to protect the genetic wellbeing of future generations.

One typical form of misrepresentation appearing in the papers concerned the scientist's statements about racial differences in the IQ of different populations. Articles repeatedly described these perfectly factual and statistical differences in IQ scores as being "theory." Today few if any scholars would ever seek to deny the fact that a measured difference in scores has existed for the several decades over which reliable testing has been taking place. Such erroneous reports, once they have appeared in print, are sent along the transmission belt, and appear again in even more exaggerated form in national magazines such as *Time*: "Virtually all scientists reject these views, of course, arguing that there is no sound evidence of intellectual differences based on race or of intellectual decline based on genetics."[22]

As if the deliberate misrepresentation of Shockley's material were not enough, his detractors sought to imply that his credibility in the scientific community was low. Columnist

21. *Ibid.*
22. *Time*, Dec. 19, 1977.

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Tom Wicker assaulted his supposedly "bigoted" theories as "repugnant." "It can reasonably be argued that — on this subject, rather than in his field of expertise — he (Dr. Shockley) is not professionally entitled to serious attention or academic credit."[23]

Ironically, if the same standards were applied by the press to the scholars who attacked Shockley how few would be given credence since they regularly sought to create a false sense of genetic-environmental dichotomy. By misrepresenting advocates of genetic research as "genetic determinists" politically motivated critics were often able to leave their readers with the totally false impression that Shockley and other scientists whom they chose to label "genetic determinists" denied *any* role to environment. This, of course, was patently and ridiculously untrue.

But to fully understand the energy behind the attacks on Shockley, we must realize that the political stakes were high. With good access to the media pulpit, it was possible to discourage the further research and dialogue which Shockley advocated. Such research, he pointed out, offered humanity the chance of ensuring a sound future for posterity by reducing dysgenic trends if the role of genetics in determining personality was as strong as he and other experts believed; or conversely, if it were negligible, would forever dispel false notions about the hereditary component of intelligence.

As is true in so many fields covered by the press, an overly small stable of "expert" sources often becomes the exclusive source of "authoritative" information for the mass media. Should those regularly quoted form the sole basis for discussion, the opportunity for true distortion — whether in international politics, economics, health sciences, etc. — is obvious. This is precisely what occurred in many of the media reports on Shockley. Using Marxist Stephen Gould's book, *The Mismeasure of Man*, as a launching pad, the press also came to call upon a heavily politicized publication, *Science for the People*,

23. *Ibid.*

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as a source in its stories concerning IQ, Jensen, and Shockley. Richard Lewontin, another Marxist featured on a 1975 PBS documentary, also emerged as a key source, and proved eager to provide copy for reporters. Suspiciously, the media seldom if ever cared to recognize the Marxist commitments of these favorite sources, referring to them more usually as distinguished and respected authorities, whose views could be accepted as free from the bias which they attributed to Shockley. One other academic became a key figure. A certain professor at the University of Illinois at Urbana-Champagne, by the name of Jerry Hirsch, was to become perhaps the most forceful of the anti-Shockley critics. Hirsch had authored several treatises which strongly – many would say "bitterly" – attacked those who scholars such as Arthur Jensen who questioned theories which attributed an overwhelming predominance to the role of environmental factors in human personality development. This stance made him a hero quoted with adulation in Marxist student pamphlets and by the Progressive Labor Party, and an attractive news source for a media keen on "exposing" Shockley's alleged bigotry, but hardly ensured that Hirsch himself was neutral in the debate.

The California scholar was aware of some fair treatment, and acknowledged it. *The New York Times* treatment of Shockley's appearance before an NAS audience in Durham, N.C., in a 1966 story "Nobel Winner Urges Research on Racial Heredity" was balanced, reporting facts without editorial judgement.[24] But he soon learned to expect the critical "scare" headlines and hyperbolic verbiage used in too many articles ("Nobelist Shocks 'Em Again," proclaimed one *San Francisco Examiner* headline).[25] Nevertheless, Shockley never shunned the opportunity to speak his mind freely to the public, even though he made increasing efforts to protect himself against media misrepresentation.

Professor Shockley believed that it was within the power of the media to bring pure science to the foreground. "I believe,"

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24. *New York Times*, "Nobel Winner Urges Research on Racial Heredity," Oct. 18, 1966.
25. *San Francisco Examiner*, "Nobelist Shockley Shocks 'Em Again," Oct. 30, 1968.

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he wrote in February 1969, "that one of the most valuable services the press can perform. . . is to convey. . . .the status of knowledge as appraised by Jensen. . . .[if they would do this] a useful and accurate analysis meaningful to the average reader could be achieved. . . ."[26]

Eventually, however, the level of reporting sank so low that Shockley determined to sue one major newspaper. Despite the fact that he had extended every courtesy to its reporter, and had protected himself by taping the entire interview, *The Atlanta Constitution* printed a totally outrageous article accusing him of Nazi-like theories.

Attacks On Campus

The colleges and universities were alive with ferment in the late 1960s and in the first half of the 1970s. This made them ideal places for the radical Left to launch salvos against William Shockley. The education establishment was left with little recourse but to sanction the silencing campaign against the physicist. They risked the wrath of not only the radical Left, but of a multitude of liberals as well of activist minority organizations that had been heavily influenced by radical Leftist propaganda. To enrage minorities on the campus would have been politically embarrassing and financially damaging, since the political fallout could permanently damage a faculty member's reputation, a department's standing within the university, and even the prospect for future grant funding from government or leading tax-exempt foundations.

A review of the situation at Stanford University in 1972 demonstrated how difficult it was to overcome the barrage levelled against one of its own. Shockley sought to expand upon the work by Jensen and others by discussing what he termed "raceology" in the standard curricula. To do so, a formal class would be offered, albeit on a very limited basis. Opponents contented that the topics were too far removed from Shockley's own area of expertise. Therefore, opponents

26. WAS letter to Lou Boccardi, director of The Associated Press, Feb. 15, 1969.

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requested that the professor abandon his quest to teach the subject.

One of the favorite techniques of those who united to disparage Shockley's competence in the field to which he altruistically, and without hope of personal gain or advancement, turned his attention after his co-invention of the transistor, was the allegation that his academic training, which we must assume indicated only the classwork he performed as a young man in order to obtain a Ph.D. — a very elementary achievement in comparison with Shockley's academic and scientific achievements — was not in the social sciences. One calls to mind the comment by Dr. Johnson, as recorded by Boswell, that a man of high intellectual capacity can walk just as well up one hill as another.

Shockley was a genius, of that there can be little doubt, and what his detractors failed to mention was that he was an expert not only in scientific method, but that he was a brilliant mathematician. In an age when social scientists are struggling to achieve scientific recognition by introducing rigorously statistical approaches to their studies, some barely understanding the pitfalls that lie in wait for the mathematically poorly trained, Shockley brought one of the world's best mathematical minds to the social sciences, and devoted his own superbly trained scientific mind to the study of the heritability of IQ for more than 25 years of his life.

It would not be unfair to say that, quite apart from the fact that he undoubtedly possessed an exceptionally superior mind, Shockley spent more time studying the specific details of the question of the heritability of intelligence than many of his less gifted detractors have been able to give to research in the social sciences in general during their entire lives. The reason he gave so much of himself to this subject was well known to his colleagues. As he said to many, he considered his research into heritability and intelligence to be of far greater importance to humanity than his work in physics, or the invention of the transistor. The entire future of humanity depended upon the willingness of each successive generation, including his own generation, to pass on to posterity an

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adequate and healthy genetic heritage. So dependent has mankind made itself on science and technology, and so radically has mankind altered the global environment, that a posterity low in intelligence would have a very poor future so important has intelligence become to us.

It was by no means certain that Shockley's request to teach a course on heritability and IQ at Stanford fell into any "gray" areas, since countless examples exist within the Ivy League of academics handling course instruction in fields outside their immediate purview. Lincoln Moses, a dean at the graduate school at Stanford, was entrusted with setting up a committee to overview the situation. He appointed a five-member faculty group, whose mission was to explore the propriety of Shockley's teachings in biology and genetics.

In time, the group reported that Shockley should indeed be entitled to carry out the assignment. Guidelines were that it be given for a quarter, or semester, and with no formal credits granted to enrollees. Dean Moses' response startled unbiased members of the faculty at Stanford and at other universities. He overturned his own committee, although Shockley was permitted to use campus facilities for lectures.[27]

Even honest liberals who would normally be expected to be unenthusiastic about Shockley's area of research were dismayed. "Who is trying to kid who?" asked renowned Harvard law professor and activist Alan Dershowitz. "Does anybody really believe that a Nobel prize winner who had devoted considerable time to studying a 'relevant' subject would be denied the opportunity to give a graduate elective (course) — even on non-credit basis — if the subject were not so controversial?"[28]

Dershowitz, noting that "the values of free speech and academic freedom are under attack from both right and left," correctly identified the most serious of the far Left's charges, namely, that Shockley preached "genocide." In a syndicated column, the Harvard professor — by then known as a staunch

27. *Detroit News*, Sept. 15, 1974.
28. *The Stanford Daily*, "Disservice to Academic Freedom," May 15, 1972, p. 2.

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civil libertarian — asked "how dare serious academics use the word 'genocide' to describe Shockley's teaching? Are they really so ignorant that they do not understand the meaning of that word?"[29]

Not surprisingly, the openly Marxist Students for a Democratic Society (SDS), kingpin of the organized Left during this period, found in the soft-spoken, diminutive academic a suitable target around which to rally their forces. SDS, through its ability to work with other extreme and more innocent student groups, would serve as a catalyst for picketing, mobilization, and for forms of overt disruption undertaken by the various militant black groups. These organizations were well equipped with the methodology of such disruption, and with other effective means of intimidation. They had shown their power by literally closing down hundreds of campuses; they were a permanent fixture, and capable of coordinating activities on a nationwide scale. In particular, they had sought to exacerbate anti-white feeling amongst American minorities. The far Left included black militant organizations as well as New Left groups, both claiming that capitalism was rooted in racial repression.

SDS was well-funded, highly organized, and commanded considerable sympathy within specific faculty and administrative circles on many "prestige" campuses. SDS was capable of generating considerable press attention, and for years prided itself on creating news by deliberately staging colorful dramas well suited to the media of television.

It was against this type of politically-motivated muscle that Shockley was forced to wage a protracted war. Shockley was invited to speak before Brooklyn Polytechnic Institute in New York City in 1968. This school's symposium was to have concerned intelligence. The tumult which erupted would form a pattern for years to come: When Shockley attempted to address some 500 — mostly scientists — he was shouted down by a small cadre of militants, probably fewer than fifty.

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29. *Ibid.*

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There is proof that the anti-Shockley movement was well-organized. A relatively scant number of anti-Shockley activists existed at each of the universities where he spoke, but there were always militant students who could be bussed in from other places, as well as borrowed from off-campus political organizations, for large scale disruptive large scale protests if these were necessary to shut down any selected campus. The Far Left SDS was well organized in Greater New York, as were their allies in black militant groups and among the New Left, all of which were simultaneously espousing an "anti-racist" theme as among their major *raisons d'etre*. The solitary Shockley was an easy political target.

In October 1969, over four years after the first nationwide publicity grew out of the *U.S. News & World Report* interview, a speaking engagement at Dartmouth College drew attention from radicals. Given its proximity to Metropolitan Boston – home of 26 colleges and universities – Dartmouth would be a perfect place to present the then familiar spectacle of campus disruption.

Shockley's autumn speech on dysgenics and factors affecting human intelligence was seen by the far Left as a welcome opportunity to draw in more publicity in its self-styled war against "capitalism" and "racism." As Shockley reached the stage, experienced Marxist-directed demonstrators rioted, threatening the feeble campus security forces and intimidated onlookers. Using the time-proven shouting-down technique, the minority prevented the hundreds who attended from hearing Shockley's speech.[30]

The Dartmouth faculty and administration – which prides itself to this day for its genuinely liberal tendencies – was disgusted. Other liberals decried the violent tactics of the protesters. "The cause of academic freedom suffered a severe setback," noted *The New York Times*, which warned that "pressures to politicize the universities are pernicious," regardless of

30. *The New York Times*, "26 At Dartmouth Face Hearing for Demonstration," Oct. 24, 1969 p. 40.

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their origins.[31]

Shockley always requested "objective analysis" of his views in his appeals to disruptive elements to allow him to proceed with his guest lectures – many of them costing colleges a paltry contribution of under $100 in expenses. An invited Brooklyn Polytechnic Institute lecture was fearfully canceled by that institution when it was deemed likely to become the occasion for violence by radical protestors.[32] Other faculties and administrations on campus were equally perturbed – and few could resist the enormous pressures they encountered from the radical Left when faculty or students sought to hear Shockley speak.

The threat of violence came, of course, on the heels of violence and destruction wreaked upon academia by SDS during the peak of its power. Reminiscences of those spectacles were powerful enough to serve as subtle pressure for years to come. In 1973, at Staten Island Community College (also in New York City), echoes of Brooklyn Polytech were seen. College president William Bierenbaum invited Shockley to speak in what was described by *The New York Post* as a seminar, part of a series that featured an array of controversial guests. Indeed, Bobby Seale, chairman of the Marxist Black Panther Party, was among those on tour at the public-supported campus. Charging that "the ruling class" sponsored Shockley as part of a "national movement of racism in the universities," a leader of the (far Left) Progressive Labor Party indicated turmoil would ensue if the scheduled engagement took place.[33]

Much to its credit, and a source of great hope for the future, the all-black music radio station in New York, WWRL – among the most popular stations in that city's metropolitan region – chose to place itself on record as supporting Shockley's right to speak under terms of society's free speech covenants.

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31. *The New York Times*, "Free Universities – Or Captive?", Oct. 20, 1969, p. 44.
32. *The Wall Street Journal*, "The Lysenko Syndrome," May 22, 1968, p. 16.
33. *The New York Post*, Apr. 19, 1978.

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Forcing the cancellation of lectures by Shockley, arranged at the invitation of students and faculty, became a common and effective tactic. At Princeton University in April 1975, a debate with Roy Innis, chairman of the activist group known as CORE (Congress of Racial Equality), nearly kept Shockley off the grounds at Old Ely. When Shockley made it to Yale, a band of 70 students and non-students succeeded in shouting down those at the podium, forcing Shockley to abandon hopes of communicating with the audience of several hundred.[34]

Disruption was the response by political extremists in other instances when Shockley was allowed to reach the speaker's platform. What *The Sacramento Journal* termed "a violent disruption" occurred at Sacramento State College in 1971.[35] As was true in the other cases where violence took place, equally controversial speakers were given space and time, without incident. The Left knew what it was doing when it targeted Shockley but allowed others to speak.

While the termination of the Vietnam War reduced some of the power behind the SDS, attacks on Shockley remained fertile ground for the New Left activists. It was SDS that instituted a ban on the professor in Autumn 1973, forcing Harvard and Radcliffe to take a scheduled Innis-Shockley debate off the program agenda.[36]

At Case-Western University in September 1974, violent heckling and disruption again prevented an airing of the professor's views. The University of Texas saw a similar disruption and heckling incident and in March 1974, Shockley was forced to leave the stage at the University of Georgia. A year afterwards, at the University of Kansas, the scene of strident youths marching toward the stage was enough to convince Shockley and campus authorities that danger was imminent unless he departed from the auditorium.

Media coverage of the campus disturbances appeared objective when it came to accounts of the physical conflict, or

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34. *Ibid.*
35. *The Sacramento Journal*, June 1, 1971 p. 1.
36. *The Harvard Crimson*, Oct. 24, 1973, p. 2.

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melees, that took place. But they were biased and distorted the facts when it came to reporting Shockley's views. For example, in the November 1971 incident at Sacramento State University, the physicist was described as one who "believes that blacks are genetically inferior to whites in native intelligence."[37] No further comment on, nor discussion of, his research and documented conclusions appeared in that or in other comparable articles reviewing the disruption. When *The San Francisco Examiner* carried its story, the paper noted that 450 students were packed in the lecture hall, and that as many or more waited outside, since loudspeakers were to carry the presentation live.[38]

It was not the Black Student Unions at different schools that usually took the initiative, according to many accounts of the anti-Shockley protests. BSU is generally considered an amorphous, partially-organized network of college blacks, both radical, moderate, and otherwise, who seek a range of goals and lobby on a plethora of issues. Only at Sacramento State was a small contingent of the BSU responsible for the outburst. In fact, "the president of the BSU grabbed the microphone" as the scientist was about to give his talk.[39]

At times, lesser known radical groups distinct from the SDS led the charge. Shockley's own class at Stanford was invaded by The Third World Liberation Front in 1971, and a bombastic statement issued by leaders of this fringe group was in keeping with assaults seen elsewhere. "We, the third World peoples, have found Shockley racist, not only for his writings and speeches, but also in his actions....." Among those "actions" were allegedly "race theories to make killing the future generation of black and other poor people legal."[40]

A so-called Third World Coalition Against Shockley surfaced briefly under radical Left tutelage at Stanford in

37. *San Francisco Examiner*, "Blacks Halt Geneticist's Talk," Nov. 23, 1971, p. 11.
38. *Ibid.*
39. *San Francisco Examiner*, "BSU Blasted For Preventing Shockley Talk," Dec. 15, 1971, p. 53.
40. *Palo Alto Times*, "16 Invade Shockley Class," Jan. 19, 1972, p. 2.

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1972, but their largest rally, according to press accounts, drew a typically meager – yet vocal as well as potent – crowd. "The group of about 80 marchers. . .burned Shockley in effigy. . .before dispersing," noted the respected and highly professional student newspaper.[41] Other fringe groups of the far Left the Revolutionary Community Youth, described by *The San Francisco Chronicle* as "a minority faction of SDS,"[42] came to the fore in seeking to prevent any future invitations for Shockley after Harvard's cancellation of his invited lecture in 1972. Associated Press stories of the April 1972 controversy at Harvard revealed that it was SDS, however, which remained enough in the forefront to claim "credit" for posters which identified the Nobel Prize winner as "WANTED: Dead or Alive"!

While some prominent civil libertarians came to his defense, those "free speech" advocates closely linked with the organized Left never followed suit. In February 1970, the American Civil Liberties Union was concluding its review of the Dartmouth anti-Shockley protests, and actually corresponded with Shockley at his office. After four months of enquiry, however, they asked him to comment on their report within five days – during the week of Washington's birthday. This time frame made it all but impossible for him to comply, and their report and evaluation of the fundamental First amendment issues at stake were never to include his perspectives.

The October 1973 decision by Harvard to prohibit Shockley's appearance demonstrates a response which was typical of muzzled universities. "The realities and exigencies of a less than free intellectual climate," stated a (Harvard) Law School memo circulated on the 18th of October, "outbalanced the desirability of our making a stand for freedom of speech."

So, the rallying cry of the anti-Shockley contingent – that he had, as Hirsch proclaimed, "taken up the segregationists' cause" – was used to silence a discussion of the reality of dysgenic issues. Capitulation became the common response on

41. *The Stanford Daily*, "350 Turn Out At rally Asking Shockley Ouster," Feb. 17, 1972, p.1.
42. *San Francisco Chronicle*, "SDS Faction Demands Action," Apr. 1, 1972, p. 2.

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most major campuses. Perhaps his own students and colleagues associated with the Foundation for Research and Education on Eugenics and Dysgenics, which Shockley founded to carry out the vital education and information components of his work, were the most accurate observers. They called attention to popular distortions which, because they affected his public image, allowed a campaign of silence to endure. The "oversimplified and exaggerated perceptions," along with "sensationalized" racial components of his thesis, led many "peers in the academic world" to avoid the fundamental precept of the intellectual inquiry which is essential to scholarship: "the crucial need for open-mindedness."

Shockley's Actual View of Blacks

In examining the attacks on Shockley, it is important for serious students of the issue to understand Shockley's actual view of blacks. Most of the harshest criticism levelled against him concerned alleged "racism."

Shockley was quick to distance himself from bigotry. The issues, as he correctly perceived them, affected all races, and he pointed to both anecdotal as well as statistical information on whites and their socio-economic status in order to drive home that message. The barrage directed against him, as he correctly implored observers, led social science and science to ignore a critical question. "A consequence," he warned in 1978, "is that the dysgenic threat to blacks is overlooked."

"The fact that black Americans are educationally and socially disadvantaged," he wrote, " causes nobly-motivated — but wishful-thinking — intellectuals to vehemently oppose demands, like mine, for the evaluation of the role of genetics in social performance. A consequence is that the dysgenic threat to the blacks is overlooked. Census Bureau reports reveal that this threat is real: Black women college graduates average only 1.9 children, not enough to maintain their fraction of the population, whereas black rural farm women (near the bottom of the socio-economic ladder) average 5.4, nearly three times as many. (For whites, the threat is less: 2.3 and 3.5.) I have not found comparable statistics for

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transgenerational AFDC families but fear that they would be even more threatening, as suggested by the factor of six that I deduced from Professor Segalman's percentages."

What he and others termed "the tragedy" of American blacks remains apparent today in the dysfunction of inner city communities from coast to coast, which appears to follow the pattern prophesied by Shockley in his call for an investigation of human dysgenics. Many blacks "themselves suffer the misery of . . . the tragedy," he wrote, citing statistics in unemployment, educational attainment, and other basic skills.

He was concerned that improper remedies to the social ills affecting lower class, and particularly black Americans, were to be self-perpetuated. Welfare programs, he felt, were leading to the form of dysgenics viewed as crippling to underprivileged Afro-Americans. "Untold harm" was being done by dysgenic trends within the black community, he cautioned a scientific conference in 1975. But generally accepted statistics record American blacks, as a group, averaging about fifteen IQ points lower than whites. Research has established that *cultural bias does not explain* lower black IQ scores and, furthermore, that IQ scores do statistically predict educational achievement and do so as well for blacks as for whites.

Thus, the IQ deficit means — again only statistically — a deficit in educational achievement and, hence, inferior jobs, less pay, lower social status, and other characteristics of the less successful black Americans, as detailed in NAACP leaflets. Shockley never for a moment argued that other factors, of an environmental nature, did not contribute to Afro-American deprivation where it existed, but he did emphasize that unless the IQ gap could be overcome by eugenic means, the elimination of massive areas of black poverty would be impossible. This tragedy would grow if the dysgenic facts which he identified were permitted to progress toward their logical conclusion. While large numbers of professional and entrepreneurial Afro-Americans today enjoy high incomes and many accumulate great wealth by any standards, this same group is tending to produce fewer children in line with the practices of the white population, while the welfare families

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and drug addicts are producing more than their share of the next generation of American blacks. To the extent that the successful, professional blacks were blessed by a higher IQ than those at the more prolific bottom rungs of black society, this would only serve to increase the IQ gap between the black and white segments of the American population in future generations. For wise remedies to develop, responsible scientific investigators of proven sincerity using acceptable premises must be provided with adequate facilities to do the diagnosis.

Biographical Notes

The man who launched the phrase "dysgenics," describing it as "the mechanization of self-destruction for civilizations," simply took the data provided by Jensen and others equally concerned that "retrogressive evolution" would create far greater social problems than existed in the turbulent years of the 1960s and 70s. A prejudiced individual would have denied the humanity, or perhaps more appropriately, the inhumanity, of conditions confronting the blacks. Professor Shockley was obviously no such bigot.

Born in 1910, Shockley directed the U.S. Navy's research on anti-submarine operations during World War II. Returning to Bell Laboratories as director as solid-state physics research in 1945, he was part of a three-man team which, in 1948, perfected the first transistor. He was awarded a Nobel Prize for his efforts some eight years later. As one whose contributions to science – and especially to the then-infant field of electronics – he was accustomed to challenging standard beliefs, and in the tradition of the competent scientist he was able to quickly grasp the scope of any profound shifts in the making.

Leaving Bell Laboratories in New Jersey to become president of a semiconductor firm, Shockley developed and became the proprietor of many fundamental patents still widely used in the field of electronics. To some extent he may be described as the founder of Silicon Valley. But his talents and interests did not lie in the business world, and as many of his employees left to found their own firms, Shockley chose to return to pure research and teaching a rising generation the

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disciplined techniques of scientific enquiry. Consequently he accepted a position as professor of electrical engineering at nearby Stanford University in 1963.

Shockley's odyssey began with his reflections about a 1963 news item, concerning a low-IQ criminal who threw acid on another member of the San Francisco citizenry. The perpetrator of this heinous crime was one of 17 illegitimate children — all white — whose mother had an IQ of 55! Low-intelligence populations, he perceived, could multiply at excessive rates, crippling the ability of society to care for them and to provide what amenities the truly stupid were incapable of providing for themselves. Realizing what this meant for the future of humanity, he began to devote an increasing proportion of his time to studying intelligence, heritability and demographic trends in intelligence.

It was in 1965 that the results of his research, and his efforts to attract the attention of the academic world to the significance of this area of enquiry, exploded into controversy. An interview with *U.S. News & World Report* followed a speech he gave to Gustavus Adolphus College in Minnesota. The address covered heredity and its role in variation in intelligence. The interview which followed allowed him to expound before nearly 400,000 subscribers of what was then the third major news weekly magazine in the U.S.

In that interview, he was drawn into a discussion of black performance in IQ tests, but more importantly, he discussed dysgenics and the disastrous potential for its continuation over time. He noted that a "genetic role of the dice" had repercussions for both individuals as well as for societies and for posterity. As mentioned before, his notions contradicted much of what was offered by the then dominant forces of environmentalism, the views favored by political liberals, who had succeeded in generating additional billions in domestic social welfare spending programs — most of which not only withstood the "Reagan Revolution," and continue to increase their drain on the national budget to this day. (In fact, as columnist Ben Wattenberg observed in 1983, the share of the federal budget devoted to such programs decreased by only a percentage

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point, falling to 53% in Reagan's own first three budgets, compared to 54% during Carter's last two budgets.)

By the time the 1965 magazine interview appeared, Shockley was prepared to move to the offensive, and two years later he urged the National Academy of Sciences to carry out an investigation into dysgenics. The gauntlet was dropped before him, and he had little option but to take on the challenge. Harsh, unsubstantiated press attacks grew during the late 1960s, and he defended himself by carrying a tape recorder with him at interviews. Even phone conversations with the media were similarly recorded.

Professor Shockley's attempts to communicate his views to the general public were consistently thwarted by the media. This saddened him, because he knew that the future of humanity depended upon sound research into the relationship between genetics and intelligence, and on the ability of each succeeding generation to bequeath an adequate heritage of intellectual qualities to posterity. Many civil libertarians readily took to his cause. The media's misunderstanding and often surely deliberate distortion of his writings and pronouncements came largely through the politicization evident in far too many attacks. When he went on the offensive, and developed an outreach, media bias prevailed. Unable as most reporters were to understand his sophisticated mathematical approach, the *Los Angeles Times* remarked, "his writing is . . . somewhat statistical, reading like a scientific treatise . . . and packed with phrases that do not enhance clarity."[43] Shockley, who had a highly developed if somewhat wry sense of humor, did not endear himself to the media when he retaliated by inviting future reporters seeking interviews to submit to standard intelligence tests, to prove that they were capable of comprehending his explanations!

It was hard for him to forge alliances. He had no quarrel with abortions — a fact which made him unpopular with many conservatives. He battled William F. Buckley, who joined

43. *The Los Angeles Times*, "Shockley - Fame and Controversy," May 25, 1972, p. 1-4.

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forces with liberals in the groundless attack on Shockley's supposed "racism."

The fact that, as he said in 1969, blacks were "born. . . enslaved in a slum environment"[44] made little impact on those committed to the frenzy of Leftist social activism prevalent at that time. Environmentalism "commands the heights of the 'social sciences'," said columnist Mike Culbert in 1970. Its supporters were "wary of the incursions by those few upsetting voices suggesting hereditary quality – genetics – is at least 80 percent of intelligence and certain success-getting attitudes that go with it."[45]

Shockley rightfully pointed to society's interest in "the quality problem," as he told the Cleveland City Club in 1975: "Dysgenics is the name for down breeding, for retrogressive evolution, or population pollution, caused by excessive reproduction of the genetically disadvantaged."

"If my fears about this threat are true, the taxpayer will suffer. But those who will suffer most are the babies, born in slum environments with statistically poor heredity from unfair shakes from the badly-loaded genetic dice cups of their parents. Few of these babies will reach the mainstream of society. The remainder will be, in effect, genetically enslaved for their lifetimes. Although I endorse welfare programs to reduce this misery, I hold that society has a moral obligation to analyze this potential genetic disaster. My faith in humanity supports my belief that establishing relevant truths will lead to truly humane courses of action."

The data he used to make this claim came from standard census sources which pointed to comparative rates for different groups of both whites and blacks. With rural black women accounting for 5.4 children apiece, compared to 1.9 for black women college graduates, the message is abundantly clear – and obviously has to do with human intelligence, not race.

There was a common theme in all the attacks on Dr.

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44. WAS press release, Apr. 28, 1969.
45. *Berkeley Daily Gazette*, "Shockley In Third Test Before Nation's scientific Elite," Apr. 27, 1970, p. 8.

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Shockley. "After delivering an address to the American Psychological Association in Washington, D.C.," according to *The Sacramento Union* in 1971, "he was publicly accused by some delegates of 'racism' and promoting 'fascist' ideas associated with Nazi Germany."[46]

Support from honest liberals emerged. Not only did civil libertarians, as well as legal experts such Harvard's Alan Dershowitz, come to his defense on matters of principle, others knowledgeable in the population fields felt his inquiries to be justified. One was former Alaska Senator Ernest Gruening (a Democrat). In a June 1971 letter to then-senator Joseph Tydings (Dem.-Md.), Gruening then a consultant to the Population Crisis Committee, wrote that Shockley's attitude was simply that "not enough emphasis was given to the quality of mankind ... Without diminishing the emphasis on the quantitative aspects, the qualitative should have increasing attention, a view I fully share."

During the battles with the National Academy of Sciences (NAS), in the face of strong media and Left wing attacks on the physicist, a few recognized the underlying issues. Referring to the close-minded attitude of the NAS bureaucracy, Congressman Charles Gubser (Rep.-Cal.) insisted he was "shocked that men who call themselves scientists are afraid to seek the truth."[47]

Misinterpretations were widespread throughout the entire course of the Shockley crusade. Commenting on a 1968 newspaper article, Shockley himself noted that he was quoted as stating that blacks' "rapidly rising birthrate is the cause" of the problems associated with lack of assimilation on the part of a healthy share of Afro-Americans.[48]

Anyone cognizant of the actual statements and tenets of his writings would realize immediately that he never implied that at all, and that he continuously reckoned the cause, as well as

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46. *The Sacramento Union*, "Blacks Break Up Lecture at SSDC," November 23, 1971, p. 1.
47. *The Congressional Record*, July 15, 1971.
48. *The San Jose Mercury*, March 14, 1968, p. 30.

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the symptoms, of many social ills affecting blacks .

Support was more widespread than was evident in the 1970s, largely because of the press blackout and other media distortions. With many leaders of the liberal-Left establishment such as Yale University President Kingman Brewster firmly in the anti-Shockley camp, a number of academics and scientists dared not confess their true sentiments. After all, their dependence upon grants, foundation support, and even acceptance by the "mainstream" media would be jeopardized should they take a courageous, principled stand.

Many times, Shockley proclaimed that others would join renowned scientists such as Sir Andrew Huxley (the British Nobel Prize winner) — who vigorously supported the thesis behind dysgenics — were it not for political pressures.[49]

It was difficult for people in sensitive positions to rally to the cause of a man repeatedly portrayed as a bigot, on a par with less educated racists and extremists. As has been seen in previous sections, many leading newspapers echoed the thoughts of *The San Francisco Chronicle*, when it described him as "the controversial scientist who thinks black people are born mentally inferior."[50]

Yet, some rays of truth permeated through the smear campaign. Currents of truthful analysis were so great, when it comes to this subject, that most respected scientific journals vindicated Shockley. In 1975, for example, *Modern Medicine* discussed the issue in purely scientific, rational, and non-political tones.[51] The Snyderman-Rothman study has since called attention to the widespread majority support presently given to the general thesis surrounding the heritability of intelligence.

The NAS Incident

Shockley's direct challenge to the scientific establishment

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49. *Palo Alto Times*, "British Nobel Laureate Rises To Shockley's Defense," Sept. 1, 1977, p. 2.
50. *San Francisco Chronicle*, "Shockley's Eugenics 'Bonus' Plan," May 18, 1970.
51. *Modern Medicine*, Feb. 1, 1975.

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reached a crescendo in when he proposed that the National Academy of Sciences (NAS) undertake an investigation of its own into "human quality." The ensuing debate deeply embarrassed many sincere scientists.

Five successive attempts from 1967-72 pitted the physicist against not only the politically-motivated bias which had infiltrated the scientific establishment but also against many of the politically-oriented institutions which, partially influenced by political liberalism but almost invariably responsive to media pressure, held sway over science. It is worth remembering that NAS, as a federally-funded entity, was subject to the same pressures as were felt by congressmen. In order to keep a Democratic-controlled, liberal-leaning Congress pleased, NAS – whose funding depended on Congressional largesse – would certainly not wish to defend Shockley once he had been crucified in the media. The uproar from militants, the New Left, but more importantly, from the media and establishmentarian liberals, would overwhelm the Academy.

W.D. McElroy, chairman of the biology department at Johns Hopkins University, expressed the perspective of many respected intellectuals. "I did not disagree with your proposal per se," he wrote to Shockley in 1969. "But I felt. . .that it would be interpreted by the press and the general public in a racist way."[52]

McElroy was director of the equally prestigious National Science Foundation, equally important in the context of scientific research. Considering NAS "the nation's intellectual conscience," Shockley sought to prompt probes of dysgenics and other pertinent issues, but after resolutions were seconded, they were tabled. The Stanford professor assembled several renowned men of science such as Walter Alvarez, emeritus professor and a leader of the Mayo Foundation, as well as Nobel Prize winner John Northrop, a world-famous chemist.

Forces which created "American Lysenkoism," as he referred to it, chose to silence debate rather than confront the

52. Letter to WAS from W.D. McElroy, May 13, 1969.

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politically sensitive issues. At the second try in 1969, the Shockley resolution that research into dysgenics by an unbiased team of scientists should be funded by the nation was tabled by a 200-10 vote. The actual resolution contained language which expresses the humane, forward-looking contentions advanced by the Californian in the course of a relentless drive to smother his perspectives: "I propose as a social goal that every baby born should have a high probability of leading a dignified, rewarding, and satisfying life regardless of its skin color or sex. To understand hereditary cause and effect relationships for human quality problems is an obligation of the scientifically responsible brotherhood. I believe also that this goal can best be achieved by applying scientific inquiry to our human quality problems."[53]

The third attempt in 1970 saw a counterattack launched against the Californian by a minority faction within NAS. That group sought to censure Shockley, who actually achieved — against considerable odds — support from a specially appointed NAS committee. The investigating NAS committee members declared it "proper and socially relevant" to undertake the very studies which the much-smeared Nobelist had proposed.[54]

Yet, the larger contingent of delegates at the NAS assembly voted to "receive" that committee suggestion, yet simultaneously not accept it.[55]

It was not until a year later that the Academy agreed to establish its own seminar on behavioral genetics, although the method of selecting participation on that body was left unclear.[56]

As we have seen, the tones which characterized the most vicious anti-Shockley diatribes which surfaced at SDS-spawned rallies were matched by fallacious press reports. The NAS controversy gave the less reliable elements in the press yet another

53. *The Indianapolis Star*, "Genetics and Intelligence Facts Needed," editorial, August 24, 1969.
54. *The Washington Post*, "Scientists Debate Race Intelligence," Apr. 29, 1970, p. A6.
55. *Ibid.*
56. *San Francisco Chronicle*, "Shockley wins Partial Victory," Oct. 18, 1972, p. 4.

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opportunity to misrepresent Shockley as a bigot. It was Shockley's "inner psychic drive," declared *Newsweek*, "essentially racist in expression," which propelled the NAS resolutions.[57]

Interestingly enough, Dr. Philip Handler, president of NAS in 1971 and generally considered a leader of the anti-Shockley forces, has himself been accused of "going out on a limb" in at least one other arena of scientific examination, thus indicating his inherent acceptance of the logic of Shockley's position, even if he denied its political palatability. While pondering the question of whether humans will become more akin to technological beings, complete with an array of artificial parts (such as biochips), Handler told one writer in 1989 that "man may have guided his own evolution in the sense that a human being combined with a computer represents a new species."[58]

The Atlanta Constitution Case

In 1981, one of the country's premier daily newspapers, *The Atlanta Constitution*, offered its readers a story on Shockley which contained a number of inaccurate and one-sided accounts. The scope of this single article's fabrications prompted the scientist to launch a $1.25 million lawsuit.

The piece, resulting from an interview with the Californian, described his work in dysgenics as a "demagogic hobby," while "rubbish" was used to characterize his highly detailed research findings. "Thus," said Shockley in the case's aftermath, "readers of the article will remain ignorant"[59] of the mathematical analyses he frequently prepared for lectures and for journalists. No reference was made to Shockley's expertise as a mathematician, to the advanced statistical methods used to calculate evidence for dysgenic trends, nor a paradigm to link IQ scores with his notions, or those of Jensen and Hernnstein. The "fact that this scientific tool is available for research on hereditary factors in racial differences" was

57. *Newsweek*, May 17, 1971, p. 13.
58. *The Futurist*, Sept.-Oct. 1989, p. 19.
59. WAS press release, July 29, 1981.

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deliberately, he felt, kept from readers.[60]

In its place was substituted a barrage of highly editorial remarks. Because so much of his research was omitted in any form, Shockley, with considerable justification, accepted the notion that this reportage was a "hatchet job," and one far more sinister than those appearing in other outlets.

As an example of distortion, he noted in his lawsuit, initiated in July of that year, the allegation that his views were directly traceable to those of Adolph Hitler. "The article contains the most unwarranted derogatory presentation of my position that I can remember," declared the Stanford professor. Left out of the story was his call for voluntary participation in a eugenic program available to members of all races (and initially supported by private funds on a trial basis). Described as an "amateur geneticist," he was portrayed as a prejudiced racist throughout the content, and no attempt to secure balanced coverage was evident.[61]

The trial in front of a jury in Atlanta, Georgia, provided the media with ample opportunity to defame Shockley still further. A local television station took the opportunity to invite the distinguished professor to an allegedly unbiased debate on his views, which was patently rigged in advance against him. Not only was the panel selected from amongst hardened radical agitators and one or two scholars who had made known their opposition to Shockley's thesis, but the audience was clearly imported, complete with banners and whistles, to heckle so vociferously that only Shockley's opponents could be heard, and he himself was never permitted to speak above their uncontrolled howling. Finally, a cloth was thrown over his head, in full view of the camera. Such was the way in which a licensed American TV station sought to treat one of the nation's greatest scientists and benefactors. It should be noted that Shockley, the scientist whose mind was accustomed to combat on the frontiers of science, kept his dignity throughout the proceedings, while everyone else – including the two

60. *Ibid.*
61. *Ibid.*

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"anti-racist" scholars (one a psychologist and one a young historian) who had been brought along to debate him, but in fact did little more than make angry defamatory allegations – seemed caught up in the infectious atmosphere of the riotous proceedings.

The result of the long drawn out trial, which personally cost Shockley some $80,000 in costs, vindicated his position when a mixed race jury found in his favor. But although Shockley's case against the libelous Atlanta Constitution article was found to be valid, he was awarded $1 compensation, and no costs! In point of fact, those who had libelled him won the day, and future scholars learned that if they dared to emulate this great American scholar, they could expect similar if not worse treatment by a media which had nothing to fear from the law.

The Upright y-axis

Contrary to popular myth, Shockley was no hardened Rightist – or even politically minded. He is on record as having endorsed the controversial Head Start program, stating that the remedial assistance provided to blacks was important, and that there indeed was a need for the environmental forces so critical in human development to be publicly supported.

The accusations of political bias launched against his fundamentally apolitical style by Marxists and those who share the same viewpoint as Marxists, are perhaps best answered by the anecdote which Professor Arthur Jensen of the nearby University of California at Berkeley, who knew Shockley well, likes to recount:

> "I have seldom known anyone as non-political in his manner of thinking or motivation as Bill Shockley. He was actually rather irritated by such terms and concepts as 'Left-Right' and 'Liberal-Conservative.'"

Jensen went on to recount that:

> "One night at a dinner party at which I was present with Shockley and several others, someone said to Shockley: 'Bill, I just can't figure you out. On some issues, such as your advocacy of liberalized abortion

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laws, you seem to be on the Left and take an extreme liberal position, and on other issues, such as your interest in eugenics and belief in the importance of heredity in human quality, you seem to take a Rightist or very Conservative position."

"Shockley looked a bit annoyed by this observation and replied rather impatiently: 'My position on various issues may seem inconsistent to you, but it's because I simply don't operate on the lowly X-axis of Left-Right or Liberal-Conservative. I operate entirely on the upright Y-axis.'

"'And what is that?' his questioner asked.

"Shockley replied: 'The application of scientific ingenuity to the solution of human problems.'"

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INDUS WRITING

Subhash C Kak
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Harappan civilization is viewed as one of the major cradles of civilization. The antecedents of this civilization have been traced back to at least 6000 B.C. in excavations at Mehrgarh, Harappan, or Indus, writing has been studied for over sixty years, and the possibility of its relationship to the Brahmi script (800 B.C.) was pointed out very early (Gadd, 1931, Langdon, 1931, Hunter, 1934); but until recently no systematic study of this connection had been carried out. This was partly owing to the belief that the Aryans arrived in India later than 2000 B.C. and therefore a relationship between Indus and Brahmi, which is so well suited for Indo-Aryan sounds, was impossible. But now it is generally accepted that whatever the origin of the Harappan civilization, the later Indian civilization shows remarkable continuity with it. Furthermore, the earliest Dravidian inscriptions (in Tamil) are also in Brahmi. In recent articles (Kak, 1987 a.b., 1988, 1990) I now have sought to show that there are structural connections between Indus and Brahmi. I present a summary of that part of my analysis which is based on frequency considerations to the readers of this journal here, and also present some new arguments. For an analysis related to case-endings see Kak (1988).

My analysis is based on the data available in the Mahadevan concordance (1971). Even though this concordance is not exhaustive, it suffices for a broad analysis. Pages 746-752 of the concordance give the frequencies of Indus signs. We will now list the counts for the most frequent signs including the instances when they appear as components of "compound" signs. This is as in last paragraph on page 133 of Kak (1988). But in view of the identity established in Kak (1990) of and the counts of these two signs have been added. Furthermore due to the evidence that ∪ may stand for 5 as well as , this

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sign has been added back to the list. The only signs that have been left out are those with vertical strokes for which there are grounds to take them as numerals (Ross, 1938, Kak, 1990). Also the sign with two short vertical strokes that appears to be a part of an introductory formula has been left out. The exact counts are now:

Table 1

sign											
count	2245	1254	837	459	411	406	343	256	243	292	233

We now list the frequencies of the most frequent Sanskrit consonants for a data base larger than that used by Whitney, which was the basis of my earlier analysis (Kak, 1988). We show that our conclusions do not change – which increases our confidence in our work. These new frequencies are due to Vavroušek (1986):

Table 2

consonant	r	v	t	n	y	m	s	d	p	ḥ	ṣ	ś	ṃ	h
count	2737	2710	2686	2290	2089	2063	2044	1427	907	776	763	725	707	691
Brahmi														

If one were to assume that a primitive alphabet may not distinguish properly between ś, ṣ, s, or m, ṃ or h, ḥ then the rank order of consonants will be:

Table 3

consonant	ś ,ṣ,s	m,ṃ	r	v	t	n	y	ḥ,h	d	p	bh	k
count	3532	2770	2737	2710	2686	2290	2089	1467	1427	907	591	589
Brahmi												

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Note that this set is the same as Whitney's excepting for *bh*. The order of some of the consonants is changed, but that is not surprising considering how close many of them are in their frequencies.

The significant fact about the most likely Indus and Brahmi characters is that they do look very similar in form. In particular, the two leading consonant groups in Table 3 are almost identical to the leading Indus signs. This is what one would expect if the core of the Indus signs was eventually transformed into Brahmi. The most common Indus symbol could have been transformed into the Brahmi *s*, as there is evidence of intermediate forms in use in post-Harappan cultures of the 2nd millennium B.C. (Lal, 1975). The *fish* sign of Indus is *m* of Brahmi turned upside down. The third most common Indus symbol is out of that rank in Brahmi by a percentage of less than 3, and thus could very well represent *t*. Two other signs that appear unchanged in the two groups are the Brahmi *v* and *p*.

The above conclusions are reinforced by comparisons with comparable written texts in Brahmi (Thaplyal, 1972). Most of these later seals end in the genitive *-sa* or *-sya* which would explain the much higher frequency of the Indus . For a discussion of issues related to case-endings see Mitchiner (1978), or Kak (1988).

II

Let us now turn to numerals. It is generally agreed that the signs with vertical strokes represent numerals, the exceptions being the small vertical superscripts of one or two strokes (see for example, Mahadevan, 1977), which appear to be part of beginning formulae and are probably syllables. The other numeral signs can also sometimes represent syllables as Ross (1938) has convincingly argued. But apart from the sign for 7, this is relatively rare. The frequencies of the various numeral signs as well as and are shown in Table 4.

The interesting fact about this table is that the frequencies of numerals above 5 (excluding 12 which may represent a word like dozen or month) are much lower than that of 1, 2,

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Table 4

sign												
frequency	149	365	522	136	66	41	10	20	6	70	323	381

3, 4. This forces one to assume that there should have been a symbol for 5 which when written together with the lower numerals would have stood for 6, 7, 8, 9 and so on. An analysis based on frequencies shows that only ∪ can be that symbol. That it is not a measure is established by the fact that it is not correlated at all with ||||| which must have been an alternate, and less popular, representation of 5. The correlation frequencies are given in Table 5 (read from row to column):

Table 5

∪	7	78	124	58	0	0	0	0	10	1
	5	67	20	5	0	16	1	9	6	7

Note that ∪ is not correlated with alternate representations of 6, 7 and so on whereas it is correlated with itself. This reinforces its reading as 5. Likewise an analysis of suggests that it may have represented 10, though this is less clear.

Also note that Brahmi 5 and 10 were represented as ∪ and ∝.

III

I believe I have made a case for further investigation into the relationship between Indus and Brahmi in my papers.

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There is increasing acceptance of the view that the later Indian civilization evolved indigenously [see, for example, Shaffer (1989)] going back to a tradition that can be traced back to at least Mehrgarh. In view of this, it is unreasonable not to evaluate the most logical hypothesis of Indus evolving into Brahmi. Without regard to other theories we must allow the data itself to provides the clues for further progress. Whether Indus language was Indo-European is questionable, but there appears to be evidence that demonstrates connections between Indus and Brahmi. These connections can be investigated without making any assumptions about the underlying language.

I close with remarks by Raymond Allchin quoted by Goody (1987), discounting a Semitic origin for the Brahmin script, which are of general interest here. These were brought to my attention recently by Jim Shaffer: "There is a powerful argument against the idea that the Brahmi script was derived from a Semitic borrowing. The whole structure and conception of the script is quite different and must, in my view, have had an independent genesis. When an attempt was made to adapt the Aramaic script to writing an Indian language (*i.e.* in the Kharoṣṭhi script), Aramaic character was retained and continued to dominate the script for many centuries. Yet in the attempt there is a clear Brahmi influence discernable; therefore, I have always thought, the Brahmi script must have been there before the arrival of the Aramaic. In recent years I have been leaning towards the view that the Brahmi script had an independent Indian evolution, probably emerging from the breakdown of the old Harappan script perhaps in the first half of the second millennium B.C."

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INDO-EUROPEAN PSYCHOLOGICAL CONCEPTS AND THE SHIFT TO PSYCHOPHYSICAL DUALISM

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Three Indo-European works – the *Rig Veda*, the *Iliad* and the *Odyssey* considered together, and Beowulf – are examined for their psychological concepts and terms. These are mostly connected with body functions that are the locus of the psychological functions yet are not tied to the body and, as such, have no ("soul") but *psūchḗ* ("psyche") and to some extent *sawol* that are free powers that are not tied to the body and, as such, have no psychological characteristics. In later centuries *psūchḗ* took over psychological meaning from *thūmós* and then was further developed by poets, Presocratic philosophers, and eventually by Plato and Aristotle. The Greek terms exhibit no psychophysical dualism and do not obtain it until Hellenistic times. "Soul" and later "mind" obtained transcendental meanings under the influence of Christianity.

Among the heroic epics of the Indo-Europeans that were eventually written down and survived to the present are the *Iliad* and the *Odyssey*, *Táin Bo Cualnge*, the *Rig Veda* (containing only elements of an epic) and the later *Beowulf* and the Icelandic sagas. The most fruitful of these for psychological concepts are the *Iliad* and *Odyssey* with the *Rig Veda* and *Beowulf* also providing some limited information. These I will consider here.

Rig Veda

Because of the almost sole ceremonial function of the Vedic chants, they reveal few psychological concepts, but some terms are relevant. *Manas*, often translated as "mind" or "spirit", refers primarily to covert acts, especially thinking. It derives from a Proto-Indo-European root *men-*, *man-*, *mn-*[1] meaning to "think", "remember" or "intend" as does English "mind". (The original English meaning was also "think", "remember", or "intend".)

Bhawe (1960) observes that "*manas* in the RV means just

1. The root *mn-* give rise to "menomics" from Greek *mnemonikos*, memory.

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'thinking' or just 'thought' and not 'mind'" (p. 51). Affect is regarded as a function of the heart, and *manas* is also a function of the body but not of any particular organ except that it is located in the heart. It shares some affective functions with the heart, perhaps because of the location. The localization of particular behaviors in particular organs is probably based on observation of changes in these organs when actions are performed; it is widespread among peoples of all periods. There is no psychophysical dualism. Behavior is a life function closely associated with breathing and other organic processes and is one with other parts of nature. *Atman*, which later became the source of covert psychological acts, was used in the *Rig Veda* as simply breath or wind.

Iliad and Odyssey

Before examining these works it is useful to note two forms of life-power that often occur among hunter-gatherers and early (Hultkranz, 1953; Bremmer, 1983). One is a power that is largely free of the body; it is involved in sleep, fainting, and ecstasy. This free power is individuality of the person and is passive when he or she is awake. It is immortal and after death can exist in another form such as an animal. The body power, on the other hand, is a life-principle that is active while the individual is awake; it is often identified with breath and is involved with such psychological activities as perceiving, thinking, and feeling. (It should be noted that the division between these two types of powers and their respective characteristics vary from one group to another).

In its original Homeric usage *psūchḗ* ("psyche") is a free power and has no psychological meaning. It is related to *psūchein*, meaning "to breathe" or "to blow". *Psūchḗ* refers to the last gasp of breath; the loss of *psūchḗ* is the loss of life: "the end of death enfolded him and his soul [psūchḗ] fleeing from his limbs was gone to Hades" (*Il.* 22.362).[2] Even the threat of its loss threatens life: "I suffered woes at heart [thūmós] ever staking my

2. References to the *Iliad* and *Odyssey* are to the bilingual Loeb Classical Library (Homer, 1919, 1924).

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life [psūchē] in fight" (*Il.* 9.322). *Psūché* is similar to "expire" in its double sense of "to exhale" and "to die". Also, to faint is to breathe out one's *psūché*. Bremmer (1983) suggests that *psūché* had a distinct meaning of breath at an earlier time but in Homer was midway toward losing it as it evolved toward a free power. Peters' (1967, p. 166) lexicon entry of *psūché* for Homer as the "breath of life" may be slightly off the mark.

At death one loses one's *psūché* which then becomes the mode of existence in the afterlife where, as an *eidolon* (shadow), it looks like the individual. It is nonsentient and has no feelings, reason, or other psychological attributes. It simply engages in rather mechanical actions of flitting about and uttering squeaks. Homer also describes the dead as looking like and moving about and speaking like the living person. Perhaps these disparate descriptions can be reconciled by the fact that when Odysseus descends into Hades the *psūché* regained rational speech after drinking fresh blood from sacrificial animals (*Od.* 11. 147 seq; 218 seq.).

Unlike the free power *psūché*, *thūmós* is active when one is awake. It is the source of affective behavior, especially when it is impulsive and irrational; *e.g.*, "thou dost not well to nurse this anger in thy heart [thūmós]" (*Il.* 6.326), "his heart [thūmós] was seized with fear of the spear" (13.163), "Achilles was driven by his lordly spirit [thumos]" (20.174). *Thūmós* gives rise to anger, joy, pity, fear, grief, desire for revenge, and feelings of friendship. It is often synonymous with desire. It is etymologically related to breath, smoke, and vapor;[3] and as breath it was the experience of aberrant breathing: the individual "panted with eagerness, choked with rage, sobbed with grief, sighed with

3. *Thūmós* is apparently cognate with Latin *fumus*, and Lithuanian *dūmas*, "smoke" as well as Slavonic *duša*, "breath". Onians (1954) suggests that *thūmós* originally referred to the vapor resulting from warm blood. Harrison relates it to Sanskrit *dhūmás*, "smoke," and to the common meaning of the Sanskrit root *dhū-* and Greek *thū-* meaning rapid movement: from that root comes Sanskrit *dhūna-*, "to agitate;" Greek *thúein*, "to seethe," and *thúella*, "to storm." Smoke's rapid movement was probably the association for the Sanskrit *dhūmás* and Greek *thúein*, "to become smoke" or "to burn." These associations also give rise to [*thúmian*] 'to vaporize,' [*thúos*] 'incense,' and [*thúmon*] 'thyme,' a plant that gives off a fragrance...."(p.65).

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regret" (Harrison, 1960, p.66), although the breath concept had largely given way to other associations by the time of the Homeric writings. Like *psūchḗ*, *thūmós* leaves the body at death and in fainting. In dying one breathes out one's *thūmós* ("gasping out his life [thūmós]", *Il.* 4.523) or *psūchḗ* ("when from feebleness I have breathed forth my spirit [thūmós]", *Od.*. 5.468). *Thūmós* resides in the chest (*Il.* 4.152) where it is contained in the *phrḗn* (sing.) or *phrénes* (pl.). *Phrénes/phrḗn* occurs 350 times in the Homeric epics, mostly in connection with psychological activities. What organ, if any, it refers to is uncertain; but it lies in the upper torso or in the midriff and could have referred to the lungs because the *phrénes* are around the beating heart, and a spear thrust into the upper back tears free the *phrénes*. Later, in the Hippocratean corpus, the plural form of the word became the diaphragm.[4] The *phrḗn* has an intellectual function of deliberation or prolonged thinking: one comes to know in the *phrénes*. In addition wine makes the phrenes reckless; a fool is not-*phrḗn*; a cheerful person is well-*phrḗn*. The last represents the affective role of *phrḗn/phrénes* that occurs along with its intellectual role. In affective functions it is more often the self-reflection on fear and grief and such feelings as awe, contentment, joy, and appreciation of beauty than the irrational impulse that *thūmós* presents. Affect and contemplation are both functions of the *phrḗn* because the two are closely related in Homeric thinking." Thinking is not dispassionate but involves anxiety, longing, and apprehension.

Another body power is *nóos* (later, *noûs*). It is both rational power and life energy ("with judgment [nóos] did she ply the lash", *Cd.* 6.320) and resides in the chest ("having in their breasts mind [nóos] and spirit [thūmós]", *Il.* 4.309). *Nóos* is an immediate understanding or appreciation of something (Furley, 1960; Harrison, 1960; Randall, 1960) rather than deliberation. The understanding it refers to is often related to the senses, especially to vision – a grasping or comprehension in the sense of

4. "Schizophrenia" is either split lungs, split midriff organ, or split diaphragm, though it could be interpreted as split deliberation. However, in later Greek, *phrenētikós* meant madness, and that also contributed to "schizophrenia".

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"seeing" or being "illuminated" and the "seeing" of a solution to a challenge. It can also be the idea itself. *Nóos* and the verb *nóein* often refer to planning. Deliberation, however, is a function of *thūmós* or occurs in the *phrḗn*, an indication that the two may be interchangeable. But they differ in that "the [phrēn] calculates whereas...the [nóos] visualizes" (Harrison, p. 75). Affect can interfere with actions of *nóos* or — more often — *nóos* gives rise to affect. *Nóos* is never a part of animals. Perhaps this latter distinction is the source for Aristotle's demarcating of *noûs*, in his hierarchy of *psūchḗ*, as the potential that humans have but animals do not.

A third body power is *kardia* (or *kradíē*, *kḗr*, or *ḗtor*), the heart. In common with widespread usage in cultures at all periods of time the heart is the seat of affect: "I fear for the Danaans, nor is my mind [ētor] firm...and my heart [kradíē] leapeth forth from my breast" (*Il.* 10.94). It differs from *thūmós* in that it lacks "an intellectual content" and is "rarely the seat of desire or hope" (Bremmer, 1983, p. 63).

A final body power, *menos*, is associated with impulsive acts and rage (*Od.* 24.520; "check thy rage [menos]", *Il.* 1.282). It is located in the *thumos* ("put might [menos] into his heart [thūmós]", *Il.* 16.529), *phrénes* ("courage [menos] in his heart [phrénes]", *Od.* 1.89), or chest ("fury [menos] in my breast", *Il.* 19.202). Like *psūchḗ* and *thūmós* it has some allusions to breath (Bremmer, 1983, p. 60). Although the word derives from the same Proto-Indo-European root *men-*, *man-*, *mn-* as Sanskrit *manas* and English *mind*, the Greek meaning has clearly changed radically, though it retains a psychological meaning.

Phrēn and *nóos* are primarily associated with cognitive behaviors, each of a somewhat different type, while *thūmós*, *menos*, and *kardía* are primarily affective behaviors that somewhat overlap in meaning. Part of the overlap of as well as the several words for heart probably reflects traditions of several subcultures, each with varying beliefs. Some of the overlap may also indicate a lack of interest in distinct categories. The emphasis on lungs, breathing, and heart probably arose from the observation of the prominent functional changes in them under conditions of affect and from the evidence that speech gives for reason,

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together with the dependence of speech on breath.

In Flowers' (1983) study of Germanic psychological concepts he found a vocabulary consisting of affective, cognitive, and breath references. These are similar to the Homeric body power terms that comprise cognitive and affective behaviors and *psūché* which had a nonpsychological meaning that was probably originally connected with life-breath. Both *psūché* and the various psychological terms that Homeros used are consistent with the naturalistic outlook of other Indo-Europeans.

Subsequent to Homer, Hesiod shifts the Homeric meaning of *psūché* from death to life itself, life-energy having been a characteristic of *thūmós*, and in the sixth to fifth centuries BC the lyric poets begin to give *psūché* the affective meanings that had belonged to *thūmós*. The first instance is that of Pindar in his *Pythian* in which he attributes courage to *psūché* (1.47). Gradually the poets and then other writers attribute more and more affect to it although it continues throughout the 5th century to still refer to dying and death (Burnet, 1930/1968). The Presocratic philosophers began using *psūché*, *noûs*, and other psychological terms in connection with the cosmos in ways that they derived analogically; this lead to anthropomorphic and hylozoic qualities that were later attacked. Peters (1967) observes that "the first generation of philosophers...still subscribed to most of the poetic conventions...and drew heavily upon this epic vocabulary" (p. ix.). The philosophers struggled with the separation of *aísthēsis* (sensing) and *noûs*, but it remained for Plato to make a clear distinction and for Aristotle to work out a systematic analysis in which *noûs* and *aísthēsis* were various forms of *psūché*, *psūché* being the actualization of the body's various potentialities (*dúnameis*).

Beowulf and later English

This epic was written about 700 A.D. but contains much that is in common with other Indo-European epics and with a way of life in northern Europe that was characteristic of earlier periods elsewhere. In this epic as in others psychological acts are often associated with regions or organs of the body. Thoughts are from the breast (1. 2818) and are apparently closely connected

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with soul (*sawol*) which departed from the bosom at death (11. 2819-2820). Similarly, keeper of the soul" (1. 1742) means intellect or reason. Piercing of the soul in battle (1. 801) refers to destruction of life, and one who is soulless (*sāwullēasme*, 11. 1406, 3303) is simply lifeless. When the water monster bites Beowulf he is bloodied with his life's stuff or life's gore (*sāwuldr-ĭore*, 1. 2693). In lines 184, 2422, and 2820 the usage refers to afterlife with little elaboration. It appears that in its meaning as loss of life and afterlife the word has a usage in common with Homeric *psūchḗ* for a free power meaning, but the body power meaning connected with life and thinking and reasoning are also present. These usages point to the soul not as a transcendental entity as we now use the term but (a) as life itself (or loss of life), (b) blood as life, and (c) life actions of thinking and reasoning. Although "soul" is now used as a supernatural entity and as a translation of Greek *psūchḗ*,[5] neither term originally had such a meaning. Similarly, Flowers' (1983) study of archaic Germanic psychology shows that two terms used in Old Norse (*ond* and *andi*) refer to breath as life-giving or life-sustaining; they changed in Christian times to a meaning of supernatural spirit.

TABLE 1

	Psūchḗ ("psyche")	Soul	Mind
Early	loss of life, afterlife	loss of life, afterlife life and blood thinking & reasoning	intend, remember thinking
Late	immaterial entity 3rd cent. B.C.	immaterial entity 9th cent. A.D.	immaterial entity 12th cent. A.D.

5. From the affective aspect we derive "frenetic," "frenzy," and "frantic." The intellectual aspect eventually referred to a "mind" and from that usage came "Phrenology".

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Turning to another important psychological term in English, we find that the first recorded usage of "mind" (*gemynd* before dropping *ge-*) occurred in the tenth century (*OED*, 1933) with little change in meaning from the Proto-Indo-European root referring to the act of memory, thought, or intention. We still say "keep in mind" for remember, and we still occasionally hear its meaning as intention in such sentences as, "I have a good mind to do it". But we no longer hear "have a mind of" for thinking. Under the influence of Christianity, by the twelfth century "mind" often meant something nonphysical and distinct from the body. "Soul" became transcendental in the 9th century (*OED*, 1933).

Conclusion

The three terms that are still in general use today in the English language are psyche (*psūchḗ*), soul, and mind. Table 1 shows their meaning in the earliest documents and their later meaning. We cannot trace the change of *psūchḗ* here but we may note that it changed in the Hellenistic period under conditions of great social upheaval and insecurity that caused a turning toward an inward mysticism. The psychophysical dualism that it then represented was further developed by Christian formulators and carried to all parts of Europe. The influence of Christian transcendental concepts then brought "soul" and "mind" into the psychophysical fold.

The dualistic meanings we give to *psūchḗ* as "psyche" or "soul" and to *noûs* as "mind" are appropriate for Hellenistic times and later but are misleading when applied to earlier Greeks. Our forebears of European languages were not imbued with the same psychophysical dualism as is characteristic of our own mode of thought.

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THE ORIGIN OF THE ENGLISH NATION

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Contents

England in the Sixth Century
The West Saxon Invasion
The Invasion of Kent
The Saxons, Angles and Jutes in Britain
The Saxons, Angles and Jutes on the Continent
The Kings of Angel
The Early Kings of the Danes
The Age of National Migrations
The Saxons and Angles in Roman Times
The Classification of the Ancient Germani

Maps

England at the End of the Sixth Century
N.W. Germany, Holland, Etc. at the Beginning of the Sixth Century
The Northern Part of Ptolemy's Germania
N.W. Germany Etc. in the First Century

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CAN ARCHAEOLOGY AND HISTORICAL LINGUISTICS COEXIST? A CRITICAL REVIEW OF COLIN RENFREW'S

ARCHAEOLOGY AND LANGUAGE — THE PUZZLE OF INDO-EUROPEAN ORIGINS

Mark R. Stefanovich
University of California, Los Angeles

Archaeology and Language — The Puzzle of Indo-European Origins is the newest contribution in English to the field of Indo-European studies. The author is Colin Renfrew, Disney Professor of Archaeology at Cambridge University, England. Today, Professor Renfrew may well be the best know archaeologist in the English speaking world, through his numerous books, ubiquitous editorships and cameo appearances on television, and his position at Cambridge University.

Renfrew's new book will prompt much discussion, and universal interest in the still debated subject of Indo-European origins. Yet the arguments Renfrew presents are not a panacea to the conundrum. Indeed this is not the first time that a scholar of international repute ventured into the vortex of an intellectual whirlpool without being able to keep a firm hand on the tiller of evidence.

It would be interesting to know to whom this volume was geared, as it does not satisfy the linguist, nor the archaeologist and if it were meant to be a synthesis for the lay person, it misses the mark widely, for a naive reader would be grossly misled by the simplistic solutions that the author offers. It is for the above reasons that it is necessary to point out rather bluntly the inadequacies and shortsightedness of this work, inasmuch as Professor Renfrew's book may be in danger of encouraging skepticism for a multidisciplinary approach to the Indo-European problem.

In 1986 Indo-European studies became 200 years old, for it was in 1786 that Sir William Jones made the discovery that

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Sanskrit, Latin and Ancient Greek not only shared resemblances but illustrated transformations that could have resulted only from "some common source, which perhaps no longer exists." Within the next thirty years comparative grammar, as this analogous linguistic research came to be called, was firmly in place with the publication of F. Bopp's *Über das Conjugationssystem der Sanskrit-sprache in Vergleichung mit jenem der griechischen, lateinischen, persischen und germanischen Sprache* (Frankfort, 1816). This work was followed by numerous studies on different languages that established correspondences between them and led to the reconstruction of a proto-language. This proto-idiom has been modified and is still being modified by new research and is not static in scope.

Scholars built proto-Indo-European painstakingly word by word and it rests as a major achievement of 19th century scholarship. These comparisons were conducted through the lexical elements and the phonology, morphology and syntactical systems, and now form the basis of historical linguistics. During the last two centuries the emphasis of Indo-European studies and historical linguistics has changed as new research in other fields has been brought to bear on the linguistic material. It must be emphasized that today Indo-European studies is a discipline that embodies not only historical linguistics, but archaeology, anthropology, ancient history, comparative mythology, sociology, religious studies, sociolinguistics (and other subfields of linguistics) and the ancient languages of Vedic, Sanskrit, Ancient Greek, Latin, Celtic, Germanic, Gothic, Old Irish, Old Church Slavic and the remains of other palaeo-languages attested in the area from India to Ireland. The above list is indicative of both the scope and diversity of disciplines included in contemporary Indo-European studies.[1]

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1. Watkins, Calvert. "New Directions in Indo-European: Historical Linguistics and its Contribution to Typological Studies" in *Proceedings of the XIIIth International Congress of Linguists*, August 29-September 4, 1982, Tokyo. Tokyo 1983, pp.270-77. Watkins outlines briefly the most important developments in Indo-European studies over the past 25 years. He also emphasizes the importance of the "allied fields" that are making their contribution to Indo-European studies, e.g., "language and society, language and

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It is obvious that any attempted synthesis will have to rely on the work of scholars in ancillary fields. It is unrealistic to expect one scholar to operate with the same degree of expertise in all of the interrelated disciplines; however, it is the scholar's responsibility to consult the work of others and in all cases to be qualified to judge the validity of their methodological contributions. Today, scholarship concerning Indo-European studies should find the individual as an expert in at least one of the following three fields: archaeology, historical linguistics (including the ancient languages), or comparative mythology (including religious studies) with a solid grounding in the other *two* disciplines. These are the basic requisites for any valid attempt at synthesis of the Indo-European question, as it is often called. The most recent generation of Indo-Europeanists (scholars who devote their endeavors to this field of inquiry) fully aware that no one discipline can provide the solutions to the complex set of questions surrounding Indo-European, and that collaboration with other related fields is necessary have met with much success applying a multidisciplinary approach to the Indo-European problem. A cursory survey of the literature will illustrate this.

The title of this book — *Archaeology and Language* — would and should indicate the interdisciplinary use of these two disciplines in trying to unravel the problems surrounding different aspects of the Indo-Europeans and Indo-European languages; in reality it is a diatribe against the use and validity of linguistic paleontology and, by extension, a rejection of the whole corpus of linguistic evidence pertaining to the Indo-European problem. Renfrew dismisses complete bodies of scholarship offering, instead, facile or arbitrary solutions to the underpinning assumptions. For example he criticizes and dismisses the concept of a common Indo-European kinship system, and cites Jack Goody, who "has cogently argued that

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culture; language and law, language and religion, language and mythology; archaeology and prehistory of Europe and Eurasia; language, pragmatics and diachronic behavior; language and literature, poetics and verbal art, oral literature and the formula." p.275.

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such inferences are not warranted."[2] (Renfrew 1987:80) This four-page communication by Goody appeared in 1959, raising some interesting questions regarding kinship skewing which have since been addressed.[3] In any case, it would appear that Renfrew does not consider the work by P. Friedrich[4] (even though he cites it) who indicated that the Indo-European kinship system was of the Omaha (III) type.[5] An important study, not mentioned by Renfrew, is 0. Szemerényi's work which analyzes Indo-European kinship terms in detail.[6] In this work the author addresses the idea that the kinship system proposed by philologists up to 1959 was "an anthropological absurdity"[7] as stated by Goody.

It is interesting to note that Renfrew more than once counsels to the non-experts to steer clear from the field of

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2. Renfrew, Colin. *Archaeology and Language-The Puzzle of Indo-European Origins*. Jonathan Cape Thirty-two Bedford Square London, London. 1987.

3. Goody, Jack. "Indo-European Society" in *Past & Present – A Journal of Historical Studies*, Number 16, November 1959, pp.88-92.

4. Friedrich, Paul. "Prot-Indo-European Kinship," in *Ethnology* (Pittsburgh) no. 5, 1966. pp.1-35.

5. Keesing, R. *Kin Groups and Social Structure*. Holt, Rinehart and Winston, New York, 1975. esp. pp. 112-20.

6. Szemerényi, O. "Studies in the Kinship Terminology of the Indo-European Languages, with special references to Indian, Iranian, Greek and Latin". in *Acta Iranica*, Troisième Série, Textes et mémoires, Vol. V, Varia 1977, extrait. E.J. Brill, Leiden. pp.1-240. The kinship terms are broken down into consanguineal, affinal and group terms. Szemerényi rejects a single classificatory principle and advocates, justifiably, the "principle of multiple causation." p. 195.

7. *Ibid*. 196;44;205. Indo-European possessive pronoun indicates property was communal, i.e. belonged to the joint family.

> In stark contrast to the comprehensiveness of the Omaha system, the IE kinship terminology reveals shifts upward and/or downward at the two focal points only: uncle and nephew–grandson. p.183
>
> [and]
>
> In light of the definitions given by anthropologist, Indo-European society can still be described as patriarchal if we bear in mind that this term comprises a bundle of highly variable components. It means that the society's organization was patrilineal, patrilocal, and to a considerable extent patripotestal. Succession was regulated in the male line of inheritance, as far as the primitive situation and the restricted nature of private property allowed such problems to arise at all, was probably left to the individual's decision. p.206

P. Friedrich wrote a critical review in *Language* vol 56, No. 1 (1980) 186-92, indicating his disagreements with Szemérenyi.

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expertise, a valid advice that he himself does not always follow. Thus, for example,

> There is not much point in an archaeologist who is not properly trained in the linguistic field setting out to make new or challenging statements of a linguistic nature; but when we begin to speak about the groups of people who spoke these languages, and about the historical circumstances which brought about their distribution, then we are fairly and squarely within the field of archaeology. (Renfrew 1987:77)
>
> [or]
>
> That someone who can make no claim to be a linguist should dabble in such matters certainly smacks of temerity. (Renfrew 1987;263)

Unfortunately these counsels prove to be little more than rhetorical caveats and we must take care to preserve our vigilance as we consider the author's sweeping statements regarding the subject matter at hand.

It is not possible to rigorously delineate the exclusive domain of each discipline in relation to Indo-European studies. Language and its semiotic systems and subsystems are reflected in the archaeological record, mundanely by written records and testimony and more obliquely by the overall symbolic system that to a great degree appears incomprehensible because of our ignorance of the coding systems, now lost, that were inherent in the original structure. However, we know that languages do not migrate — people do — and archaeology recovers their material remains and attempts to interpret them. We must not lose sight of the fact that language and people are inexorably bound together and cannot exist independently of one another.

Renfrew's solution to the Indo-European problem is that the first farmers of Europe, Anatolia and southwest Asia were Indo-European speakers, whose languages disseminated with the appearance of agriculture. But let us underline that the spread of agriculture can not account for the replacement of language in Renfrew's processual model. The hypothesis he is testing is in essence, how agriculture has spread, not *why*

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Neolithic language replaced Mesolithic language in all the areas where they came in contact. The question of why people acquire a new language as a by-product of the spread of agriculture is not even treated, despite the fact that Renfrew asserts that:

> If we agree to try and separate language, race and culture in a clear way, the starting point for a consideration of Indo-European problems must always be the study of language. (Renfrew 1987:77)

What should be addressed is the scope, existence and interrelationship of the cultural systems revealed in the Indo-European lexicon and the semantics of their vocabulary, and how this is reflected in the archaeological record. It is specifically this point that has made glottochronology a weak tool for determining the age of linguistic strata.

Professor Renfrew devotes seven pages (Renfrew 1987; 113-19) to glottochronology. He states that this is a "recent development in the field of historical linguistics" which has "great potential relevance to the historical understanding of particular languages and language groups." (Renfrew 1987: 113) But vocabulary and rate of change have been treated since the 19th century and are not new. M. Swadesh's work on glottochronology first appeared in the 1950's and has been duly criticized by both linguists and anthropologists in the basic assumption that the time depth of a language can be measured[8] by comparing the rate of language drift and losses between languages based on a selected "common" vocabulary.[9] Swadesh's model is considered unreliable and has been

8. Diebold Jr., A. Richard. "Linguistic Ways to Prehistory" in *Proto-Indo-European: The Archaeology of a Linguistic Problem – Studies in Honor of Marija Gimbutas*. Edited by Susan Nacev Skomal and Edgar C. Polomé, The Institute of the Study of Man, 1987. pp. 19-71, esp. 38-40.

9. Tovar, Antonio. "Linguistic Similarity and Its Significance: Comparative Procedures" in *Proceedings of the XIIIth International Congress of Linguists*, August 29–September 4, 1982, Tokyo. Tokyo 1983, pp.259-69. Esp. for the benefits of glottochronology for linguistic typology, but "as for the application of lexicostatistics, the greater the time depth, the more its value diminishes, especially when there are no written documents." p.261.

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virtually abandoned.[10] This is not to imply that mathematical theory has no place in historical linguistics, rather that the simplistic application illustrated by Renfrew will produce significant informational inconsistencies regarding language time depth because of the skewing resulting from borrowing between languages.[11] In any case glottochronology tries to reconstruct lexical innovations and not discover the causes of change.[12] Words do not only contain a semantic content but a cultural coding that in each context may be different and it is the perception of the speakers that significantly affects the retention of the lexical item.

The concept of Indo-European rests on the assumption that the language groups that make up Indo-European are "genetically derivable offshoots of the putative protoidiom"[13] and that if people had at a specific time or place a word for a particular object, then it follows that they must have been acquainted with the object, even though the archaeological evidence may be lacking. The melding together of these two assumptions by both archaeologists and linguists in the past were thought to allow for a solution to the logical question that followed: Where can one locate the original homeland of these Proto-Indo-European speakers? This "homeland" question has not been exclusively reserved for the Proto-Indo-Europeans but also concerns the Slavs,[14] Celts, and other

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10. Tischler, Johann. "Glottochronologie und Lexicostatistik" in *Innsbrucker Beiträge zur Sprachwissenschaft*, 11. Innsbruck. cited in S. Embleton, 1986.

11. Embleton, Sheila, *Statistics in Historical Linguistics* in the series Quantitative Linguistics, vol. 30., Studienverlag Dr. N. Brockmeyer, Bochum 1986. esp. pp. 45-67 for evaluation and improvements to Swadesh's model.

12. Guy, J.B.M. *Experimental Glottochronology; Basic Methods and Results*. Pacific Linguistic Series B - No. 75. Department of Linguistics, Research School of Pacific Studies, The Australian National University. 1980. The author outlines the difficulties with lexicostatistical methods and presents a language model tested through computer simulation. He states that, everything in the model relating to the causes of those changes (geographical location, population densities, state of the relations between communities, etc.) was irrelevant to the problem." p. 4.

13. Puhvel, Jaan. *Comparative Mythology*, The Johns Hopkins University Press, Baltimore, 1987. p.33.

14. For the linguistic arguments see: Birnbaum, H. *Common Slavic – Progress and Problems in its Reconstruction, Slavica*, 1975. esp. 220-35. Also Birnbaum, H. and P.

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language groups, and perforce their speakers. It is unfortunate that as soon as it became apparent that a methodology existed which could unravel the mystery of the proto-home, abuses of both the linguistic and archaeological data tend to be introduced in the hope of legitimating the political, nationalistic and ethnic claims of diverse peoples. In evaluating the various homeland theories that have been proposed Renfrew states,

> I believe that no coherent and plausible theory has yet been advanced. [He argues that] they [the authors of other theories] have placed too much faith in the idea of some reconstructed Proto-Indo-European vocabulary, from which some kind of word-picture of the original homeland might be put together. They have too readily assumed that a given pottery form, or an assemblage of items of material equipment, can be equated directly with a group of people and hence supposedly with a particular language or language group. And they have not adequately explained why all these languages, or the speakers of all these languages, should be wandering around Europe and western Asia so tirelessly, in a series of migrations, thus setting up the pattern of different languages which we see today. (Renfrew 1987:75)

Professor Renfrew has correctly identified a number of assumptions that have led to abuses in the location of a proto-homeland;[15] however, these abuses do not impinge on the

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Merrill, Recent Advances in the Reconstruction of Common Slavic (1971-1982), *Slavica*. 1985 esp. 65-82. These treatments are based exclusively on linguistic arguments and do not take into account to any great degree the archaeological evidence. Also Schenker, A. "Were there Slavs in Central Europe Before the Great Migrations?" in *Slavic Linguistics, Poetics, Cultural History – in Honor of Henrik Birnbaum on his Sixtieth Birthday 13 December 1985*, Edited by M. Flier and D. Worth, Slavica. Publishers, pp.359-73. Discusses briefly both the archaeological as well as the linguistic material. He also notes how emotional an issue the homeland question tends to be. "The theories placing the Slavs along the Vistula, the Oder and the Elbe at the time of the birth of Christ are not verifiable scholarly statements. They merely express the West Slavs' romantic longing for 'roots' and for genealogical parity with their West European neighbors." p.370.

15. Birnbaum, Henrik. *Linguistic Reconstruction: Its Potentials and Limitations in New Perspective*, *Journal of Indo-European Studies*, Monograph No. 2, 1977 but author states condensed version was presented at XIIth International Congress of Linguists, Vienna, August/September, 1977. Birnbaum "sets out to redefine and explore the methods of

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validity of linguistic reconstructions.[16] The reconstructed proto-language must be viewed as a complex semeiotic system functioning initially within a larger non-Indo-European social system.

Renfrew appears to believe that by disregarding the evidence offered by linguistic palaeontology, he has furnished himself with a *tabula rasa* that would allow him to construct a model that will account for the spread of language in the Neolithic, a period in Indo-European studies thought to precede the arrival of the Indo-Europeans in the areas traditionally associated with them. He stresses the fact that based exclusively on linguistic palaeontology Indo-Europeanists tended to view the Proto-Indo-Europeans as nomads; this excluded them by definition from actively participating in the more directly agricultural aspects of the Neolithic.

This idea of the roving Indo-European nomad is outdated and no longer subscribed to by most Indo-Europeanists. During the past forty years, anthropological and ethnological

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linguistic reconstruction, its functions and constraints." This is an excellent summary and critique of the attempts at linguistic reconstruction. The author also discusses the case of Nostratic and the possible existence of linguistic "macrofamilies". p.51

16. Meid, Wolfgang. "The Indo-European Lexicon and its Usage as a Problem in Reconstruction" in *Proceedings of the XIIIth International Congress of Linguists*, August 29–September 4, 1982, Tokyo. Tokyo 1983, pp.710-14. Meid cautions the use of the lexicon stating,

> the proto-language was not an ideal unified system...but a system of constraints and variables, in which even the constants (phonemic, morphemic, leximic etc.) have only an ideal validity, and variation or variability dominates in language behavior, as a result of the ability of speakers to use language creatively. This built-in variability makes it impossible to reconstruct a system which is unequivocal in all details, and claims that this can be done are illusory....What can be extrapolated with some degree of certainty is basic typological patterns and trends of development, morpho-semantic categories, paradigmatic classes and similar broad structures; the small-scale pattern cannot be determined in detail, precisely because it was nature variable ... To group and arrange these into individual systems which mirror the spatial and temporal continuum which was Proto-Indo-European is an attractive task, which however can only be tackled, by means of "internal reconstruction", when the first stage of work is completed ... To reconstruct a prehistoric lexicon ... implies at the same time a hypothesis about the 'Weltbild' which it expresses, that is its "semantics". p.711.

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research on surviving pastoralist and nomad societies has helped clarify the picture.[17] Unfortunately, earlier Indo-Europeanists had no way of accounting for the Indo-European language dispersal that took place, other than by mass migration. Although other linguistic interpretations were offered, without archaeological support they were short lived.

The author is not the first to propose that Indo-European languages[18] and Indo-Europeans were part of the Neolithic agrarian world.[19] But this stand, while possible for some geographic areas, cannot function in the overall continuum of the Neolithic world. Professor Renfrew has also been forced into a number of untenable archaeological positions resulting from his proposed solution for the spread of the Indo-European languages. Let me cite one such case,

The Etruscan language ... appears to be non Indo-

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17. Irons, William. "Political Stratification among pastoral nomads" in *Pastoral Production and Society – Production pastorale et société. Proceedings of the international meeting on nomadic pastoralism Paris*, 1-3 Dec. 1976. Cambridge University Press. 1979. pp. 361-74.

18. Georgiev, Vladimir. *Introduction to the History of the Indo-European Languages*. Bulgarian Academy of Sciences, 1981. esp. 354-64.

D'iakonov, I.M. "On the Original Home of the Speakers of Indo-European" in *Soviet Anthropology and Archeology*, Fall 1984/Vol. XXIII, No. 2, M.E. Sharpe, Inc. Armonk, New York. pp.5-87. esp. p. 65

> From our point of view there was no migration as such in the fifth-third millennia B.C.E. There was a gradual spread from one center in all directions. In the course of such a spread the group of dialects and specific isoglosses that had developed were maintained. These expanding tribes met local, poor and hungry sparser populations, often consisting of hunters and vagrant cattle-breeders. The migrants started to merge with the local population, giving them their language and cultural achievements. But in some cases, the local population may have been larger in numbers than the migrants. p. 65.

This article appeared in Russian in 1984. The model is very similar to Renfrew's. (Renfrew 1987:304)

19. Neustupny, Jiri. "Archaeological Comments to the Indo-European Problem," in *Origini-Preistoria e Protoistoria delle Civiltà Antiche*, Università degli Studi di Roma, 1976. pp. 7-15.

> And when we say that the Neolithic civilization of Southeastern and Central Europe belonged to the Indo-Europeans we do not want to assert that these were undifferentiated Indo-Europeans. On the contrary, this Neolithic period is, in our view, characterized by divided Indo-Europeans belonging to the individual European Indo-European groups. p.15.

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> European and hence, on the theory developed here, probably of longer standing in Italy than its Indo-European neighbors. (Renfrew 1987:67)

This is difficult to accept since it would identify the Etruscan language with a pre-Indo-European Mesolithic substratum in northern Italy and there is no archaeological basis for this. V. Georgiev, who maintains that Etruscan is Indo-European, is in a minority, but bases his conclusion on the idea that the Etruscans came from Asia Minor. He utilizes the Lydian language, which is now accepted as Indo-European as a *tertium comparationis* to prove the Hittite-Luwian origin of Etruscan."[20] The archaeological evidence seems to agree with the Etruscologist Pallottino who, among the most prominent, has maintained the autochthonous and Non-Indo-European character of the Etruscans.[21] In any case, maintaining Etruscan as a non-Indo-European language indigenous to northern Italy in the Mesolithic is too nebulous an assumption to be considered seriously at this stage, based either on archaeological or linguistic evidence.

The subheading "The Languages of Europe" (pp. 67-70) covers the Italic languages, Celtic languages, Germanic languages, Slavonic languages, Baltic languages, Illyrian, and non-Indo-European languages of Europe. This feat is achieved schematically with the aid of defective diagrams[22] and inappropriate generalizations. The diagram for the Slavic (Renfrew uses the English term Slavonic) languages would indicate that Old Church Slavic is a direct descendant of Common Slavic and that the later breakup into Western, Eastern and South Slavic stems directly from Old Church Slavic. However, Old Church Slavic is only "the earliest attested written language of the Slavs and, in fact, scarcely more than the first literary recording of a Late CS [Common Slavic] dialect, namely,

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20. Op. cit. Georgiev 231.
21. Pallottino, Massimo. *Etruscologia*, 7th ed. Revised Editore Ulrico Hoepli Milano, 1984. esp. pp. 85-110.
22. Why Estonian and Hungarian are separate (on the end papers) when a Finno-Ugrian group is indicated is incomprehensible.

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primarily that of the Macedo-Bulgarian region."[23] Modern Slovene, the language of northwestern Yugoslavia, spoken by 1,900,000 people,[24] is not included in his chart, although it is especially important to historical linguistics as it preserves a number of archaic features no longer exhibited in the other South Slavic languages, e.g. the dual number, and the supine.

Under the rubric of Illyrian Renfrew classifies,

> The principal remaining Indo-European languages of Europe can be classed under this heading (although Thracian and Dacian are dealt with below [in one paragraph on p. 71!]). There are no texts surviving from the Illyrian language in classical times [nor from any earlier period], although Greek references do mention the Illyrians in what is now Yugoslavia and Albania. Modern Albanian may be descended from the Illyrian language, or possibly from Thracian.[25] (Renfrew 1987, 69)

This taxonomic use of Illyrian to subsume other "principal remaining Indo-European languages" smacks of pan-Illyrianism which is no longer regarded as tenable either archaeologically or linguistically. The Illyrians mentioned in the Greek sources do not refer to the entire territory of modern day Yugoslavia. Today, Illyrian scholarship employs a far more restricted use of the term Illyrian.

It is extended human interaction that is necessary for the spread of Indo-European languages, not just exchange networks, culture contacts or the acceptance of the probability of independent invention. It is interesting that Renfrew who, in his earlier publications, proposed theories that rested heavily on indigenous development, now accepts diffusion as an explanation, albeit so-called demic diffusion. For example, he

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23. Birnbaum, H. *Common Slavic – Progress and Problems in its Reconstruction*, Slavica Publishers, Inc. 1975. p.69.

24. 1981 Yugoslav census figures.

25. Katičić, Radoslav. *Ancient Languages of the Balkans* in the series Trends in Linguistics – State of the Art Reports, edited by W. Winter, Mouton 1976. Two parts. See Katičić for a sober interpretation of the existing information on the Palaeobalkan languages.

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goes to ingenious ends to promote the independent development of metallurgy in the Copper Age in southeastern Europe.[26] When the uncalibrated radiocarbon dates appeared too low, he proposes the creation of a fault line,

> the whole thing is rather like the formation of a fault in geology. Inside and outside the line there is a sequence of levels – the geological strata, or in this case the well-established sequence of cultures in each region. Now suddenly along this fault line, the whole outside area slips down together, leaving the area inside unmoved. Strata or cultures which used to be on a level with each other are displaced, those of western, central and northern Europe slipping down and becoming earlier. Where the late neolithic cultures of Iberia or the Balkans were formerly on a level with the Aegean early bronze age, they slip down, coming to rest instead at a much lower level, alongside the Aegean neolithic.[27]

While this may be a convenient and even ingenious metaphor, it certainly is not an adequate explanation as to why calibration should be applied to some dates and not to others.[28] This arbitrary arrangement of data so as to make it fit a preconceived hypothesis – to accommodate the necessity for calibration in Europe as opposed to Egypt where the traditional dynastic dates were in harmony with the uncalibrated radiocarbon dates – weakens the methodology. After all of his anti-diffusionist reprobations we have come full circle to demic diffusion. *Ceteris paribus*, diffusion is diffusion. Let us be clear, there is nothing intrinsically wrong with diffusion, either cultural or demic, as an explanation for some events in prehistory.

Renfrew utilizes a wave of advance model for his

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26. Renfrew, Colin. "The Autonomy of the South-East European Copper Age" in *Proceedings of the Prehistoric Society* 35, 1969. pp.12-47.

27. Renfrew, Colin. *Before Civilization – the Radiocarbon Revolution and Prehistoric Europe*. Cambridge University Press, 1979 (with corrections and new Introduction), pp.104-5.

28. Weinstein, J.M. "Radiocarbon Dating in the Southern Levant." in *Radiocarbon* 26, pp.297-366, Recent carbon 14 dates reflect a tendency toward a higher chronology for the beginning of the Early Bronze Age at 3400 B.C.in the Levant and for the Egyptian Predynastic.

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explanation of the diffusion of the Indo-European languages in the Neolithic. This model was introduced into archaeology by Ammerman and Cavalli-Sforza and endeavors to coordinate the archaeological record, radiocarbon dating and the spread of genetic polymorphisms to furnish a model that accounts for the origins of the European Neolithic. The model makes the implicit assumption that a complete culture complex including plant, animal domesticates and pottery are being diffused together. However pottery is not necessarily correlated with domesticates. In addition, at this point it is important to differentiate between the transmission of culture systems and the replacement, imposition or acquisition of language – the wave of advance model has no explanatory power in this realm. *Linguae francae* and bilingualism,[29] common to frontier, border and contact areas are just that, fringe developments and not related to the linguistic nuclear area. Emeneau (whom Renfrew cites incorrectly in the bibliography) specifically addresses this problem.

Without an answer in firm terms to these problems posed by comparatively well-known situations, it is

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29. Lieberson, Stanley. *Language Diversity and Language Contact – Essays by Stanley Lieberson* Selected and Introduced by Anwar S. Dil. Stanford University Press, Stanford, 1981. In "Language and Ethnic Relations" Lieberson points out that,

> the sociological setting in which language contact and bilingual behaviors occurs has not received sufficient study (p.5)...If language influences cognition, then ethnic groups with distinctive tongues may respond differently in the same social situations. (p.6)...The important point is to recognize an inherent competition between languages in contact, based on the fact that the optimal linguistic conditions for the speakers of each tongue are incompatible. Since the gains that one language makes are to the detriment of the other, there is competition (p.9)...Mutual intelligibility must develop between racial or ethnic groups with different native tongues because of the economic, political and social interdependencies found in any society. Bilingualism, either in the form of learning another group's tongue or some pidgin, is therefore a necessary product of language contact....The *actual* frequency of bilingualism and the degree it is concentrated among one of the mother-tongue groups may vary greatly, however. In a dual economy, in which groups living side be side are involved in essentially different economic systems, the possibility exists of very little bilingualism since there may be very minimal intergroup contact....Basically, the behavior of bilinguals determines whether a multilingual society will become unilingual or remain pluralistic for generations or even centuries (p.12).

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> hopeless to think of knowing, without any direct evidence at all, why Sanskrit came to supersede the aboriginal languages of North India.[30] [He continues] Answers on questions about prestige and the like can only wait on an acceptable hypothesis about the bilinguals and their trans-morgrification and transmission of their second language.[31]

There are several reasons why the wave of advance model does not explain language displacement adequately.[32] Although it may provide an explanation for the spread of early farming from Asia Minor through the Balkans to Central Europe and does support the observed archaeological remains chronologically, the spread of agriculture is not necessarily synonymous with the spread or replacement of existing languages spoken by different contact groups. This Renfrew has not proved or really even tackled although he refers to it in passing. (Renfrew 1987;270)

Let us look briefly at the wave of advance model as put forth by Ammerman and Cavalli-Sforza and its proposed function.

> In the wave of advance model, population growth and local migratory activity are seen as producing a diffusionary process that takes the form of a population

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30. Emeneau, Murray B. *Language and Linguistic Ares Essays by Murray B. Emeneau*, selected and introduced by Anwar S. Dil, Stanford University Press, 1980. p.44.

31. *Ibid*. 45.

32. *Op. cit* Lieberson 15.

> The family of procreation is usually the family of socialization. This means that if both mates share the same mother tongue, then the only reasonable chance that a mother tongue will be passed on to their offspring is when both mates have acquired some second language. If the parents have different mother tongues but are bilingual in the other's language, *it is unclear what language will become the first tongue of their children*. [my italics] If the parents have different mother tongues but only one parent is bilingual, the chances are good that the mother tongue of their children will be the one language shared by the parents. It is unlikely that the parents will have different mother tongues and neither learn the other's language,...Basically, it is assumed that the tongue passed on to offspring will be a language that both parents understand, it is necessary for mates who share the same mother tongue to be bilingual before they are likely to transmit a different mother tongue to their children.

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> wave expanding outward at a steady radial rate....the model also specifies the quantitative relationships that are considered to hold among three variables: that is, the rate of population growth, the rate of migratory activity, and the velocity of the spread.[33]

The model attempts to account for movement over short distances (e.g. relocation of settlement to adjacent unoccupied land). The authors maintain that the model allows for the "migrants" to,

> maintain a continuity in their social contacts and also in their cultural context. The wave of advance model makes its contribution by suggesting how local processes such as population growth can produce what in some respects is a form of colonization without colonists.[34]

This model was developed by the geneticist R.A. Fischer in 1936. It makes two assumptions: population growth is "logistic", being active initially and then stopping when the population reaches a saturation level; and secondly, migratory activity takes place at a constant rate in time and according to the "random walk" process. Adapting this model to represent the spread of farming required the conceptualization of migratory activity in terms of settlement patterns, generated by the relocation of farming settlements. One of the genetic implications of the model is that prior to the emergence of agriculture, there would have existed substantial genetic differentiation between the peoples of southwest Asia and Europe. Consequently, the question of interbreeding is the crux of the model's assumptions, with an assumption that the rate of genetic differentiation is inversely proportional to the size of the populations and also to the migration rate.[35] This means the greatest variation will occur at the points farthest from the origin. This is in direct conflict with attested dialect geography where archaic features tend to be preserved on the fringes and

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33. Ammerman, Albert J. and L.L. Cavalli-Sforza. *The Neolithic Transition and the Genetics of Populations in Europe*. Princeton University Press. 1984. p. 67f.
34. *Ibid*. 68.
35. *Ibid*. 120.

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this is evident in the Indo-European speech community where, in E. Polomé's words,

> Germanic is perhaps the most archaic western IE language,[36] [and conversely] we must consider Greek and Indo-Aryan as reflecting the *most recent* developments in the Indo-European speech community at the time of its disintegration.[37]

Renfrew's solution to this problem rests with loan-words and his objection to the protolexicon.

> In the first place, any innovation, any discovery which results in a new product, will often carry the name of that product with it. So to find a word for 'wheel' or 'cart' or 'copper' or 'bronze' in several languages does not necessarily tell us very much about the origins of those languages, although it is of interest for the words themselves. Loan-words are to be expected in such circumstances. (Renfrew 1987:80)

This is a sweeping statement that is simplistic at best. The complexity of loanwords has long been recognized as a very important aspect of linguistics. Loanwords are easily borrowed from one language to another and back[38] but the morphological elements are difficult to borrow, e.g. verbal endings, pronouns, etc. The adoption of the loan word also depends on the level of acceptance, whether it is internal or external, and on the context. Borrowings can be in the form of calques and be indirect. Therefore, any attempt to explain away the reconstructed protolexicon in so facile a manner cannot be taken seriously.

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36. Polomé, Edgar. "The Dialectal Position of Germanic within West-Indo-European" in *Proceedings of the XIIIth International Congress of Linguists*, August 29-September 4, 1982, Tokyo. Tokyo 1983, pp. 733-42. esp. p. 741.

37. Polomé, Edgar. "How archaic is Old Indic?" in *Studia Linguistica Diachronica et Synchronica – Werner Winter Sexagenerario Anno MCMLXXXIII*, Editors. Ursula Pieper and Gerhard Stickel, Mouton de Gruyter, 1985, pp. 671-83, esp. 682.

38. The Serbo-Croatian stô ; stôl:stòla 'table' was borrowed into Hungarian asztal, and borrowed back into Serbo-Croatian as àstâl, both words are used interchangeably today. Rječnik hrvatskog ili srpskog jezika, *JAZU*, Ed. Dj. Daničić, Dio 1, Sv. 1. Zagreb, 1880. p. 116.

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Renfrew continues to develop his case by stating that,

> Simply to draw on a map the boundary of the occurrence of the various species of trees which have cognate names in the Indo-European languages does not establish for us the boundaries of Proto-Indo-European,...For the existence of cognate, or even identical words in various dialects and regions does not indicate that they all refer to the same things. (Renfrew 1987:81)

[or]

> So that when we find words related to 'birch' and 'beech' in several Indo-European languages it does not follow that the common word in Proto-Indo-European from which they where descended had the same specific meaning. [He continues] If the Proto-Indo-Europeans did indeed originate within the area, they may well have had in their protolexicon terms ancestral to the modern ones, as the linguistic palaeontologists would suggest. But if we imagine that there was an original homeland outside the boundary, and that the territories within it came to speak Indo-European languages through the processes of linguistic displacement, then they would need to develop an appropriate vocabulary after their arrival. They might well then draw upon pre-existing terms in their own vocabulary which had previously held a rather different meaning....(Renfrew 1987:81)

This is yet another example of scientific laxity.[39] It has never been advocated that one mechanically draw concentric circles on a map and eliminate or include terms from the protolexicon arbitrarily, although this has been practiced in the past. Today it is considered likely that lack of a familiar term would indicate no knowledge of the semantic concept. Let us note that Proto-Indo-European has never claimed to

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39. Haugen, Einar, "The Analysis of Linguistic Borrowing" in *The Ecology of Language – Essays by Einar Haugen*, Selected and Introduced by Anwar S. Dil. Stanford University Press, California 1972. pp.79-109. Haugen describes the "process that takes place when bilinguals reproduce a pattern from one language to another.... viz. substitution and importation." p. 105. This essay first appeared in *Language* 26. 210-31 (1950)

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have reconstructed the "complete" PIE protoidiom with a finite number of lexical items. At this point in scholarship, if we have not evidenced a reconstructible term based on the "laws" that are part of the operating methodology of Indo-European studies, it does not mean the concept did not exist, merely that we are unable to reconstruct it within the present methodological parameters. The ability to conceptually demonstrate the existence of a proto-lexicon on the basis of nomothetic phonological laws that operate in different language groups, stands as a towering achievement of 19th century scholarship.[40] This work is still going on today, as more and more complicated methodological approaches are applied to the established lexicons in order to reveal existing patterns that would round out and contribute to our concept of the protolexicon. Renfrew's argument is like saying that if an Eskimo migrated to the Gobi desert and had no word in his lexicon for desert, he would have renamed it with one of his numerous words for snow because it shines in the sun! It is possible that different groups of peoples coming into new areas did not encounter the same flora and fauna they had known previously and in fact, this was probably the case. However, if they were renaming the lexical items, they would most likely choose similar semantic constructs – you would not call a bird a tree, even in the case of a woodpecker!

It was to account for the spread of language through human agency, and not cultural diffusionism, that a model was required proposing the roving nomad as the carrier of the Indo-European languages that were eventually to cover the entire area of Europe and the Indian sub-continent (save several pockets of assumed non-Indo-European languages, *e.g.* Etruscan, Basque and the later case of Hungarian and the Dravidian languages in India). Today, we do not envisage such a stark economic or social dichotomy between the nomad

40. Collinge, N.E. *The Laws of Indo-European*, John Benjaminis Publishing Company in the series Amsterdam Studies in the Theory and History of Linguistic Science Vol. 35, 1985. This volume explains the Indo-European language laws and furnishes a copious bibliography.

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pastoralist and the sedentary agriculturalist based solely on the existence of reconstructible lexemes. Archaeological and ethnographic research in southeastern Europe has shown that short range transhumance, most likely the type practiced by the Indo-Europeans, was already in existence in the Neolithic.[41] This is especially true in the Peloponnesus where Jacobsen has proposed the Neolithic pastoralist as the carrier of the Urfirnis were characteristic of the late Neolithic in the Peloponnesus. It could also be proposed that the same type of activity existed in the hinterland of the Adriatic coast along the karst mountains of the Dinaric and Pindus mountain ranges.[42] These pastoralists often attached themselves to the periphery of existing village settlements and in the summer moved into the uplands and mountains to graze their herds.[43]

Renfrew states his position on the relationship of linguistic factors with social ones on pages 99-119. In this section the author also concerns himself with the direction of borrowings and uses L. Bloomfield's work as his guide. He quotes the following from Bloomfield:

> In all cases, *it is the lower language which borrows predominantly from the upper*. Accordingly, if the upper language survives, it remains as it was except for a few cultural loans, such as it might take from any neighbor. The Romance languages contain only a few cultural loan words from the languages that were spoken in their territory before the Roman conquest. (Renfrew 1987: 112).[44]

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41. Jacobsen, Thomas W. "Seasonal Pastoralism in Southern Greece: A Consideration of the Ecology of Neolithic Urfirnis Pottery in *Pots and Potters – Current Approaches in Ceramic Archaeology*, Edited by Prudence Rice, Institute of Archaeology Monographs, UCLA 1984. pp. 27-43. esp. 28-30.

42. Antonijević, D. Obredi i obicaji balkanskih stočara, Srpska akadeniija nauka i umetmpsto, Balkanološki Institut, Beograd. 1982.

Also Mirković, M. "Plemensko društvo i kultura dinarskih stočara" in *Predmet i metod izučavanja patrijarhalnih zajednica u Jugoslaviji*, Crnogorska Akademija nauka i umetnosti, Vol. 7, Odeljenje drustvenih nauka. Titograd, 1981. pp. 247-55.

43. The distance traveled from the base camp is usually 45 miles at most, based on modern ethnographic parallels from the areas.

44. Bloomfield, Leonard. *Language*. Henry Holt and Company, 1933. p.464. Renfrew incorrectly gives the year of publication as 1935. Footnote 15, p. 297.

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From the time that Bloomfield's work appeared in 1933, much work has been done in southeastern Europe on the substratal languages. Romanian, Albanian, Bulgarian and Yugoslav scholars have been particularly active in this area and have contributed to the view that language acquisition, regardless of social level of the dominant language, allows language borrowings in both directions.[45] This can be seen from the situation in Dacia which was conquered by Trajan in 106 AD and later was released from the Roman Empire by Aurelian in 270 AD. Another interesting situation to look at is the case of the Bulgars. Today, the Bulgarians are referred to as a South Slavic people who settled in the eastern section of the Balkan Peninsula and represent an amalgamation of the Slavic tribes who came into this area in the 6th and 7th centuries, and the groups of Turkic-Altaic tribes[46] who came from the Russian steppes at the end of the 7th century. They overpowered the Slav tribes under their leader Asparuch in 681 AD and formed the Bulgarian State. During the period from the 7th-9th centuries these two different peoples consolidated into a State that was to be reckoned with by the Byzantine Empire. In addition to the Slavs, the Thracian substratum also played an important part in this process. The important point is that the Bulgars, who actually molded the social structure into a state, ceded their language and accepted Slavic which in this case according to Bloomfield, would have been the lower language.[47]

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45. Russu, I.I. *Etnogeneza Românilor – Fondul autohton Traco-Dacic şi Componeneta Latino – Romanică*. Editura Ştiintifica şi Enciclopedica, Bucureşti, 1981. esp. 118-46.

46. Dvornik, Francis. *The Slavs in European History and Civilization*. Rutgers University Press, 1962. p. 2. "The Slavs, settled between the Danube and the Balkans, were organized into a solid political structure by the Turkic Bulgars, who named their state Bulgaria." p. 2.

Also Kratka istoriia na Bŭgaria, ed. Ilčo Dimitrov, Nauka i Izkustvo, 1983. esp. 37-46. See also Maenchen-Helfen, Otto J. *The World of the Huns – Studies in Their History and Culture*. University of California Press, 1973, pp. 384-5 for etymologies of Bulgar.

47. Solta, Georg R. *Einführung in die Balkanlinguistik mit besonderer Berücksichtigung des Substrats und des Balkanlateinischen*, Wissenschaftliche Buchgesellschaft, Darmstadt 1980. This volume is instructive in the treatment of the substratum in Balkan linguistics, and gives examples of borrowings in Rumanian esp. pp. 85-105.

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We can also note the resilience of the Hungarian language – a non-Indo-European language – in resisting change and preserving its syntactical and lexical integrity while being surrounded by Slavic speakers for more than a thousand years in their present "homeland."[48] This is not to say that there has been no influence on both language groups from each other. But in the 9th century the Magyars were a minority population in the area with a very complex ethnic makeup who had been pushed into their present living space by the Pechenegs from the Black Sea steppes.

> Renfrew feels that Bloomfield's generalizations are of, real relevance to the archaeologist. For we have no direct evidence whatever (from the time in question) about the language that was spoken in prehistoric times but we do have evidence of the social organization of past societies. (Renfrew 1987;113)

This is a curious statement inasmuch as archaeology does not furnish directly information about the structure of prehistoric societies where written testimony is lacking. The reconstruction of the prehistoric social structure is based either on modern ethnographic parallels, the applicability of which has been questioned, or on a set of assumptions that are supposedly testable. The processual model calls for a research design that has a set of arguments testable by the principles of probability sampling. Therefore, the failure to obtain a reliable sample is crucial. The quantitative variability exhibited between areas indicates that the spatial distribution of items, (isoglosses in linguistics) may vary independently of each other. There is never any difficulty with the statistical equa-

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48. Fodor, István. *In Search of a New Homeland – The Prehistory of the Hungarian People and the Conquest*, Corvina Kiadó, 1982 esp. pp. 277-88. The exact ratio of the Hungarians to the indigenous population has been put at about two to one. Fodor estimates a Hungarian population of about 400-500,000 based on a source that mentions that the Hungarians could muster 20,000 horsemen. He also states that "researchers" put the number of the indigenous population "at the most 200 thousand." p.284. Regardless of the validity of these original numbers it is certainly true that the Hungarians were surrounded by differentiated Slavs, Germanic peoples, and other peoples that were trying to assert their dominance in the area.

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tions, it is the assumptions that often beg clarity.

Explanation begins in archaeology with observations that have been linked through cultural or behavioral functions to past conditions. For example, the attempt to explain variability in burials at a given location, or regionally, has "proved" that ranking in a society can be judged by the richness of the grave assemblage and that in this context the wealthiest graves would be indicative of a higher prestige stature in the society. These assumptions do not take into account the effects of the environment or mutual effects of intersocial contact – characteristics recognized as relevant to the deceased. These assumptions rest on the value system of the society and need not be true, or observable; however, if the assumptions are true there may be no need for a complicated research design to ascertain something that is empirically observable and would result in the same conclusion regardless of the research design. The point is that the evidence for social structure based on archaeological data is as reconstructed as the linguistic evidence, except that for the non-specialist it may be more difficult to visualize a linguistic reconstruction, but that is a difficulty with the nature of the evidence, not to it's validity.

Chapter 9 entitled "Ethnogenesis: Who were the Celts? (Renfrew 1987:211-49) is one of the longest chapters in this book. Renfrew feels that his consideration with the origin of the Celts would be an appropriate example to illustrate the processual approach to archaeology and language. He states,

> the discussion...offers [in this chapter] the opportunity of considering one of the most interesting and currently neglected of topics in the whole field of prehistoric archaeology (neglected, that is to say, in the West although not by our Russian colleagues): that of ethnogenesis. (Renfrew 1987:211-12).

This is a gross exaggeration – the question of ethnogenesis has been at the nerve center of archaeological research in Europe, often in tandem with Indo-European research, from the 19th century. For example, controversies surrounded the origins of both the Trojans and Mycenaeans immediately following their archaeological discovery by H. Schliemann in

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the 1870's. Ethnogenesis is not and has not been the concern of Soviet scholars solely. Researchers, for example, in south-eastern Europe have also been concerned with the problem of ethnogenesis, especially regarding the ancient peoples of the Balkans, e.g. Macedonians, Thracians, Dacians and Illyrians and their modern successors. "In the U.S.S.R everyone has an ethnic status",[49] for this reason, academic as well as political goals have often been at the core of ethnographic and linguistic research in this "multinational state containing some 130 different ethnic groups speaking some 130 different languages!"[50]

Renfrew adheres to the definition of ethnos given by the Soviet ethnologist Dragadze whom he quotes.

> Ethnos...can be defined as a firm aggregate of people, historically established on a given territory, possessing in common relatively stable particularities of language and culture, and also recognizing their unity and difference from other similar formations (self-awareness) and expressing this in a self-appointed name (ethnonym). (Renfrew 1987: 216)

This definition embodies the limiting concept of an ethnos having to be "historically established on a given territory." Ethnicity is frequently associated with geo-political considerations regarding the legitimacy of an occupied territory.[51] Historical territorial occupation is not pertinent in defining an ethnos since territorial occupation and its legitimization vary through time, regardless of ethnic existence. The crucial question then is: Who decides what is "historically established"

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49. Comrie, Bernard. *The Languages of the Soviet Union*, Cambridge University Press, Cambridge, 1981. p. 2.

50. *Ibid.* 1

51. Haarmann, Harald. *Language in Ethnicity – A View of Basic Ecological Relations*, Mouton de Gruyter, Berlin, New York, Amsterdam in the series Contributions to the Sociology of Language Vol. 44. Ed. Joshua A. Fishman. 1986. The importance of language to the ethnic process is outlined but the author makes the notable conclusion that "language is subject to relativism or relativity in its role in ethnic processes...the findings...do not allow the analyst to establish some kind of stable relationship between the basic notions of 'language' and 'ethnicity'." p.257 and see further for detailed analysis.

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and when? The process of ethnic formation and ethnicity cannot be considered from a diachronic standpoint but must be treated synchronically with changes in concert with the development of social structure which in itself is ever-changing. Ethnicity is fluid and depends not only on the language spoken by the individual but also on the social structure in which he learns and speaks it. Renfrew goes on to note that language and ethnos are not equivalent, which is just, but he also suggests that "*ethnicity is a matter of degree.*" (Renfrew 1987:217) The level of ethnic manifestation may vary but ethnicity is not a variable.

Renfrew claims to take a processual view of ethnicity (although he never states what that exactly is in relation to ethnogenesis). He defines ethnic groups as,

> groups of people who recognize themselves as distinct, and who see this distinction as part of their birthright. (Renfrew 1987:216)

[and continues to distinguish between]

> polity may be defined as a selfgoverning group of people, generally occupying a well-defined area. This does not that they define the group in territorial terms — often the group is defined in terms of kin relationships ... Ethnicity is ... rather different, although in the modern world of nation-states the effective polity (the state) is often in effect an ethnic group also ... (Renfrew 1987: 215)

[He continues]

> Ethnicity and political organizations do not always coincide, although they often may. Many polities may together form a single ethnic group — as for instance in the case of the Greek city-states. (Renfrew 1987: 216)

Unfortunately, Renfrew often straddles complex problems by intellectual hedging that reveals itself in conflicting and contrived solutions. For example:

> it is perfectly possible, using the linguistic evidence to indicate the territories which were occupied by peoples speaking Celtic languages at about the time of Christ. It is consequently quite a simple matter to study the material culture of those times and places, and to take

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particular note of the remarkable art style which we find there, (Renfrew 1987:233)

[then]

This point simply emphasizes again that language and art style should not be too readily equated, and that their origins are likely to be very different one from another. (Renfrew 1987:237)

After an elementary sketch of the Celts, including their language, art, archaeology and references from the ancient sources, Renfrew claims that:

> The earliest Indo-European speakers will have reached these areas [Europe north and west of the Alps] by 4000 BC, although the differentiation into individual languages may have taken place very much later. But in a very real sense, the undertaking of becoming Celtic began then....(Renfrew 1987:249)

What is "the undertaking of becoming Celtic?" He continues,

> we are entitled to apply the term Celtic to the customs, material culture and the art of these Celtic speaking communities....The origins of that art and culture are to be found in the same lands and therefore among people speaking Celtic languages, as we have defined them. (Renfrew 1987:249)

Unfortunately this contradicts his earlier claim (1987:216) that "language and ethnos are not equivalent." (Renfrew 1987:216) Perhaps the Celts are not ethnē!

Chapter 10. "Indo-European Mythologies" (Renfrew 1987: 250-62). In this chapter Renfrew goes further and tries to argue that comparative mythologists have been misguided in their endeavors to attempt the reconstruction of Proto-Indo-European myth and Indo-European social structure. He explicitly disregards the assumption that,

> similarities exist because of a shared background which is not simply linguistic, in the common Indo-European ancestral language, but also cultural. (Renfrew 1987:250.) [One paragraph further he continues] Today, however, there are serious difficulties to holding such a view. For while the notion of the spread of farming, and with it of

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> Indo-European speech, may adequately explain the linguistic communality, it will certainly not do for some of these supposedly shared social institutions. In 6000 BC these were very simple farming communities and we cannot assume that they already had the sort of specialized and differentiated social structure....(Renfrew 1987:251)

This is one of the typical leaps in logic that pervades this work – set down a hypothesis, act as though it were proven, and then reject any data which does not fit into the model. No Indo-Europeanist has ever advocated a "state society" in the Neolithic. Renfrew states that,

> If we are thinking in terms of the first farmers as the earliest Indo-European speakers of Europe, in the sixth and fifth millennia BC, we are speaking of what are generally regarded as egalitarian peasants. Their societies, as noted above, probably embodied no hierarchical ordering whatever: certainly their material culture does not reflect it (Renfrew 1987: 253)

It is interesting to note that Renfrew is willing to accept the paucity of "material culture" as proof that the first farmers are "egalitarian peasants" and yet will not accept the absence of a word in the protolexicon as proof that the Indo-Europeans were not familiar with some particular object.

Although Renfrew praises the work of Dumézil as an "impressive and very copious series of works,...[that are] difficult to summarize...." he nonetheless outlines the Indo-European tripartite system in two pages and comes to the conclusion that,

> The societies described in this tripartite functional scheme are hierarchically ordered. In the vocabulary of anthropology these are stratified societies; that is to say they show one of the principal attributes of what are termed state societies. (Renfrew 1987:252)

This error in the basic assessment of the tripartite system is due to a superficial understanding of the Indo-European social structure as proposed by Indo-Europeanists. This confusion could have been put to rest if Renfrew had looked closely at C.

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Scott Littleton's excellent anthropological assessment of G. Dumézil's Work (cited by the author),[52] or the remarkably lucid and incisive synthesis by J. Puhvel, *Comparative Mythology*[53] wherein he elucidates not only the methodological assumptions for the study of Indo-European myth but their application. Renfrew has set up his own windmill and then makes these revelatory statements that should now enable the individual to see the light at long last. In this manner he is not obliged to disagree with the established Indo-Europeanists, he merely ignores them – a rather high-handed and condescending approach to the scholarship of others. He also feels that:

> if we take a processual, anthropological viewpoint, it is not the least surprising that societies on the brink of literacy in different parts of the world should show some general similarities....(Renfrew 1987:255)

It must be noted that no serious scholar has espoused the view that a "tripartite system" could only be Indo-European and no other. It merely states that all those that are Indo-European are tripartite. Quite a difference!

A few pages later he tries to find support in the work of the Soviet linguists Gamkrelidze and Ivanov[54] who proposed an,

> 'original Indo-European homeland...within eastern Anatolia, the southern Caucuses and northern Mesopotamia in the fourth-fifth millennia BCE.' (Renfrew 1987: 269)

He then asserts that

> It is therefore encouraging that archaeological arguments, which are quite independent of their linguistic

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52. Littleton, Scott C. *The New Comparative Mythology – An Anthropological Assessment of the Theories of Georges Dumézil*. 3rd ed. University of California Press, 1982.

53. Puhvel, Jaan. *Comparative Mythology*. The Johns Hopkins University Press, Baltimore, 1987. This work may have been published after Renfrew completed his manuscript.

54. Gamkrelidze, Thomas V. and Vjacheslav V. Ivanov. *Indoevropeiskij Jazyk i Indoevropejcv – Rekonstrukcija i Istoriko – Tipologiceskij Analiz Prajazyka i Protokul'tury* 1984, 2 vols. Tbilisi, Publishing House of the Tbilisi State University. English Translation of Title: *Indo-European and the Indo-Europeans – A Reconstruction and Historical Typological Analysis of a Protolanguage and a Proto-Culture*. Renfrew uses an arcane transliteration Yaztik for Jazyk or Iazyk.

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> ones, have led me to very much the same conclusion. (Renfrew 1987:269)

But this is rather absurd as the work of Gamkrelidze and Ivanov bears little similarity to Renfrew's. The only thing in common is that of an Anatolian homeland, and even that at chronologically different times. They maintain the local development of the Hittite language and the spread of Indo-European languages through a series of migrations – some via a second homeland in the area north of the Black Sea. Curiously, just one page further on in his text, Renfrew rejects his previous assertion stating that,

> this position runs, of course, against all the archaeological counter-arguments set out earlier, and cannot be accepted here.(Renfrew 1987:270)

So one wonders how the work of the Soviet authors lends any credence to Renfrew's work, when their methodology, data and conclusions are diametrically opposed to Renfrew, except in that they start from an eastern Anatolian homeland. The importance of Gamkrelidze and Ivanov at this point does not rest so much in their posited Anatolian homeland[55] but rather in their reinterpretation of the phonemic system of Proto-Indo-European and an expanded Proto-Indo-European lexicon that utilizes Semitic and Kartvelian languages for the reconstruction of the different semantic groups expressing fauna, flora, economy and material culture.

Nor does Renfrew clearly comprehend or effectively consider the "Kurgan" theory, which surely raises questions which need to be addressed. Indeed, Marija Gimbutas set out

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55. Gimbutas, Marija. "Primary and Secondary Homeland of the Indo-Europeans – comments on Gamkrelidze-Ivanov articles" in *The Journal of Indo-European Studies* vol. 13, nos. 1 & 2, Spring/Summer 1985. pp. 185-202.

Gimbutas' rejection of the proposed Indo-European homeland by Gamkrelidze and Ivanov rests on the view that, the great Neolithic civilization of this region [southeastern Europe, Anatolia, Transcaucasia and northern Mesopotamia] is the antithesis of all the characteristics understood as Proto-Indo-European, which have been reconstructed with the help of comparative linguistic and mythological research. [and]...When they [Gamkrelidze and Ivanov] refer to the cultures between the Balkans and Mesopotamia the authors use too low a chronology. p. 185.

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the "Kurgan hypothesis" in 1956 and it has been the prevailing theory for the Indo-European homeland. Renfrew erroneously derives the Kurgan tradition from the Cucuteni-Tripolye culture of north east Romania and the western Ukraine at 3500 B.C. and this "means that the initial colonization of the western part of the steppes took place from the west," (Renfrew 1987:97) and thereby theoretically invalidating the "Kurgan hypothesis." There is no archaeological basis for this reversal. The 'fault line' type argument reappears!

It is of interest that the latest work of M. Gimbutas cited by Renfrew is from 1980 and it is incorrectly cited as *Journal of Near Eastern Studies* instead of *The Journal of Indo-European Studies*. Renfrew has chosen to ignore the works of M. Gimbutas subsequent to 1980.

We now return to the fundamental issue of the working relationship between archaeology and linguistics in solving the multifaceted Indo-European question. Historical linguistics is firmly interwoven into the Indo-European tapestry and to dismiss this discipline and replace it with a model that carries no linguistic explanatory power does not bring us any closer to answering the questions: where, when and how Indo-European languages spread.

There is no doubt that the Indo-European Question will undergo many new theories, many adaptations of the old, and a broadening of the existing work, all in the hope of providing the definitive solution to a conundrum which has caused so much ink to be spilled! One may certainly applaud the attention that the Indo-European question will receive because of the prominence of Professor Renfrew, but it is to be regretted, on the other hand, that this work will most certainly foster misconceptions and over-simplistic viewpoints in this more than complex field of scholarship.

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PICTURE OUT OF FOCUS: COLIN RENFREW'S ARCHAEOLOGY AND LANGUAGE – THE PUZZLE OF INDO-EUROPEAN ORIGINS

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We may all be pleased to observe that the study of prehistory seems to be emerging from a period of relative obscurity to a brighter period of revitalized scholarly interest, as well as of a wider interest on the part of the general public. It is true that in linguistics, the trendy theoretical schools still command the greatest amount of attention (and money, at least in the United States), to the detriment of historical and comparative linguistics. But in the fields of archæology and comparative mythology, the study of the Indo-Europeans, the pre-Indo-European substrate, and the relation of each to the other and to subsequent history is increasing as we learn that there is more to discover about our prehistory than we ever knew, and that those discoveries are relevant to us today.

It is obvious that the popularization of Indo-European studies will not come from a great public clamoring to read *The Mankind Quarterly* or *The Journal of Indo-European Studies*. We will doubtless find it in the work of the writers of fantasy and historical novels. We must to hope that such works will do justice to the rigorous and excellent work which has been done on our subject, especially that of the last three decades.[1] The interest in Indo-European studies which such works generate is not only valuable for the understanding of prehistory by the general public, but it is good for the discipline as well; surely more than a few Celticists, Germanicists, and Indo-Europeanists found their way into their studies through the

1. Which they well may. I am happy to recommend to the reader as a splendid example Evangeline Walton's inspired version of the Four Branches of the Mabinogi: *Prince of Annwn, The Children of Llyr, The Song of Rhiannon*, and *The Island of the Mighty* [all 1975, from Ballantine Books, New York].

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cellar door of J. R. R. Tolkien's *Lord of the Rings*. We trust that our works will be taken as a coherent and useful body of resources for all who will refer to them. But there will be a particularly respectable, if unenviable, burden on the shoulders of those scholars who attempt *general* syntheses. Still more unenviable a burden is given a man like Colin Renfrew, who, in his *Archaeology and Language* [1987a], offers not only a general synthesis but one which opposes in large part more traditional views of the Indo-European problem.

It is difficult to describe Professor Renfrew's book. After several weeks I came to the conclusion that the only single word that I could choose is *astonishing*. Page after page I found myself astonished to read what I was reading. It is hard to think that Professor Renfrew could have been serious when he wrote this book. He has, in just three hundred pages, tried to demolish two hundred years of Indo-European studies. I will waste no further time before saying that I believe that he has failed to do so convincingly.

It is to Professor Renfrew's credit that he has tried to mate archæology and linguistics. His book is a basic, popular edition, based on arguments which are in the main rhetorical, often persuasive, but just as often not. This rhetorical focus is best exemplified in a criticism he himself makes of attempts by Finnish and Russian scholars to decipher the script of the Indus Valley sealstones. Consensus has been that they are written in a Dravidian language, an interpretation which, as Renfrew states, follows the assumption that "the Indo-European languages are intrusive into the north of the Indian sub-continent, and that the Dravidian languages were already there when they came." (p. 185) Renfrew mentions that attempts have been made to connect these inscriptions with proto-Elamite and Indo-European, and then goes on to explain that

> [the difficulty is that in each case, for a successful decipherment, one needs to start with something that is known. So far, unfortunately, there are no bilingual inscriptions involving the Indus script, nor can any proper names yet be recognized in it. So the present

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> decipherments consist in *assuming* a solution, and then trying to show that the results are plausible. This can lead to positive results, but again it can easily be an exercise in self-deception, and it is not clear that convincing progress has been made of the interpretation. (p. 185)

How true this is, and, sadly perhaps, how easily this argument can be applied to the whole of Professor Renfrew's book. His argument is that the Indo-Europeans are the original neolithic peoples of Europe, and that they arrived ca. 6000 B.C., bringing agriculture and Indo-European languages to the indigenous mesolithic cultures. (pp. 145-52) It is based on the assumption that this was the case, and the evidence throughout the book which is given to support this is either carefully culled to do just that, or is prefaced by Professor Renfrew's disclaimer that he "is not convinced" by the contrary evidence.

Professor Renfrew is not a linguist, and indeed he recognizes his own limitations in his essay. (p. 263) Introducing questions of the distinctions between language and culture, he is right to say that "[t]remendous confusion about linguistic and cultural origins has come about because language and culture have not been properly distinguished." (p. 76) But then follows the sentence: "Of course language and culture often go together". This is inadequate. The two are inseparable, two mirrors facing one another. If I learn Cherokee, I mean really learn it, as more than just a translation code for my native tongue, then I learn too Cherokee culture. The Cherokee language does not "often go together" with Cherokee culture. It is part and parcel of that culture, and without it, the heart of that culture is lost. One questions here Professor Renfrew's intuitions and his approach to the "processes" of the Indo-Europeanization of Europe. One questions further when, out of French *fait*, Italian *fatto*, Spanish *hecho*, and Portuguese *feito*, he reconstructs **fakto*. Latin *factum* notwithstanding, the evidence points only to **fakto*. (pp. 102-03) I do not believe, as Professor Renfrew does, that Mycenæan Greek "probably differed at least as much from its Indo-European neighbours

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of the time as it does from Modern Greek" (p. 177), nor do I find that he offers arguments to support this opinion. The Indus Valley script may indeed "be a mixed hieroglyphic and syllabic script, rather than a pure syllabic script like Linear B." But Egyptian is *not* "a true pictographic script". It is mixed pictographic, alphabetic, and syllabic. (pp. 183-85) These minor points indicate a general sloppiness in Renfrew's argument, and his more major arguments about language change – in general or with respect to specific cultures – do not fare much better under even casual scrutiny. If the arguments that Anatolia served as the point of origin for the wave-dispersal of Indo-European dialects, following the same routes as the dispersal of neolithic economy, are as free of difficulty as Professor Renfrew suggests (p. 150), then our isogloss maps should reflect this. Raimo Anttila's elegant attempt, at least, does not. [Anttila 1972:305]

Great attention is given the shortcomings of glottochronology. (pp. 112-19) Professor Renfrew is not the first to criticize it. [*see* Sjoberg and Sjoberg 1956; Arndt 1959; *esp*, Bergsland and Vogt 1962; Diebold 1964; Fodor 1965] Few scholars since the mid-seventies have made much use of it as a chronometric device, though achronometric lexicostatistical techniques have been and are being used to aid in the subgrouping of languages, especially when other means of making such determination are unavailable. [*e.g.* Dyen 1975:150-60, 223-30; and Greenberg 1987:1-37] But certainly the whole Indo-European question does not rest on glottochronological analysis, as might be understood by Professor Renfrew's discussion.

Great attention too is given the problem of trying to make the proto-Indo-Europeans into nomads, pastoralists, or nomadic pastoralists, and to the relations of societies with such economies to agricultural communities. (pp. 78-79, 83-84, 197-209) Richard Diebold has recently outlined "*some* of the anthropological and linguistic evidence which suggests that this imputed facet of the Indo-European migratory dispersals was/is unwarranted and that it is perhaps mischievously wrong-headed to continue subscribing to it." [Diebold 1986]

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He suggests that proto-Indo-European palæeconomy was based on 1) short-range transhumant herding of livestock, 2) non-hydraulic agriculture, and 3) hunting, fishing, and foraging. It is the second of these three which allows for the cultivation of certain crops by the proto-Indo-Europeans without implying that they possessed the full neolithic economy that we find in the pre-Indo- European Balkans, the assumption upon which Renfrew bases his argument. Indeed, it also accounts for the fact that there are Indo-European terms for only *some* agricultural realia ('barley', 'gather', 'grain', 'meal', 'mill grist', 'plough', 'sow (seed)', 'yoke', etc.), and why *non-Indo-European* terminology accounts for so much of the vocabulary of agriculture, in particular that of the cereals and legumes ('apple', 'chickpea', 'lentil', 'melon', 'millet', 'oat', 'olive', 'pea(se)', 'vetch', 'wine', 'wineskin', etc.), in the Indo-European languages. [Diebold 1986]

The social models for linguistic replacement are interesting, though somewhat two-dimensional. (pp. 123-37) There are many different levels of language contact: more or less intimate, with different levels of borrowing, bilingualism, etc. Indo-Europeans were probably not in all areas numerically "dominant", nor should they be considered always to have been "conquerors", at least not in the same sense that the Romans were. They must have been prestigious – not to say "privileged" (p. 112) – but distinctions of "upper" and "lower" languages strike me as fairly simplistic. Albanian has borrowed an enormous percentage of its vocabulary from its many "upper" conquerors (the Romans, the Slavs, the Turks), but still has not succumbed to them, and preserves intact a vital core of inherited Indo-European forms. [Huld 1984:10-12]

I cannot evaluate the statistical analyses, as I lack the training to do so, but it seems to me nevertheless that in assuming that migratory action is random in direction and that it occurs at a rate of eighteen kilometers per generation – even taken as an average – one runs into the same difficulties of arbitrariness as does glottochronology. Of course any school of thought – "processual" or otherwise – endeavoring to talk about the migrations of prehistoric people must make general-

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izations and proceed from them, but we must never forget that *we are always talking about real people*, and we must make our speculations plausible within the range of real human motivations: always probabilities and likelihoods within that manifoldness. The "process" is *never* removed from the lives of those individuals. Statistical views help us to tell the story of history, but that story is about us and our ancestors, who are much more interesting than statistics. Professor Renfrew's arguments often take the form that "there is no reason to suppose", or that "it is dangerous to assume" thus and such. (pp. 75, 80, 95, 163, 255) This is pure rhetoric. A dangerous bridge can still get you across the ravine, and though there is often no reason to suppose that it is safe, you might also be foolish to go the ten miles out of your way simply because you suppose it to be otherwise. *A priori* suppositions are always just that, and never necessarily any more than that. These caveats are truistic, but they must nevertheless be borne in mind if we are really historians and prehistorians. There is a balance between timidity and temerity.

Perhaps one of the most infuriating things about Professor Renfrew's book is that it pays almost no attention whatsoever to religion. The tens of thousands of neolithic female figurines which have been found in Greece and the Balkans are given four lines of text only half-way through the book. They "have been taken as indications of cult practice." Renfrew says that he shares this view, "although other interpretations are possible": interpretations which he does not give. (p. 154) Griffith's translation of Hymn I.130 of the *Rgveda* (p. 179 *ff.*) was made in 1889 into an archaized English; his translation in general has been described as "unsound". [O'Flaherty 1984:12n.] Why not choose a hymn with a more accurate and readable translation? Professor Renfrew describes Mycenæan and Greek religion as "very different belief systems": a shallow and simplistic, if typical, definition of religion. But here is perhaps the most important facet of human life which we will need to deal with if we wish to understand our ancestors and to evaluate what they have given us. *Religion* outlasts pottery styles and can transcend linguistic change, for in many ways it

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is the core of a culture, especially in the prehistoric period. Notions of what life is, and what death is, are the most compelling and deeply- rooted of all human concerns. Now it is axiomatic that religion is a messy business. One cannot derive laws from its action, as one can from the observation that Slavic /s/, Sanskrit /ś/, and Welsh /k/ (in many instances) all derive from proto-Indo- European */ḱ/. One cannot dig religion up and radiocarbon date it – only its artifacts. Yet the constituents of religion – sacred space, sacred time, and sacred action – can be correctly inferred from the archæological record. We can recognize Solstice alignments in Irish megalithic monuments; we can infer springtime rituals of regeneration from the bones of young animals found in hypogea and burial sites on Malta; we can recognize ritual libations on Minoan frescoes, and understand certain decorated objects elsewhere as libation vessels. For the content of the religion of prehistoric Europe, we must turn to the interpretation of what textual evidence we have: the mythology of early historic Europe and the folklore of later periods. The challenge of this *archæomythology* is great; it requires a discerning mind and a sharp intuition. It is not easy to do, and not easy to prove – especially given the difficult preconceptions which many people, scholars and otherwise, have today about what religion is and how it may manifest. Yet the archæological, mythological, and folkloric data which we have must be brought together and interpreted, and those who can do it should receive the attention of those who cannot. Colin Renfrew has done his readers a disservice by omitting some of the most important works of one individual who has pioneered this endeavor. By claiming that Marija Gimbutas' work is based primarily on linguistic palæontology (pp. 17-18), and discounting much of it on those grounds, he has distorted the truth in favor of his argument. In fact, her elegant model, based soundly on her observations of the symbols found in the archæological, mythological, and folkloric records, accounts for the radical transformation in cultic artifacts in the Balkans beginning ca. 4500 B.C., by suggesting that only a new cultural force could transform so radically a tradition which had been in place for

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some two thousand years, and by offering cogent and plausible descriptions of the two main cultures in clash. The articles by Gimbutas which Renfrew has chosen to place in his bibliography do not give a complete representation of her contribution to prehistoric European studies. [For which see, for example, Gimbutas 1980b, 1982, 1985, 1986, and 1989]

The ongoing reconstruction of pre-Indo-European life and culture in Europe helps to resolve the greatest shortcoming of Georges Dumézil's tripartite model. His Third Function suffers from its being a kind of catch-all for what does not fit into the First and Second. Farmers, craftsmen, artisans, and goddesses are left there undifferentiated, associated with production, fertility and health. Some of Professor Renfrew's criticisms of Dumézil's model are well made. (pp. 256 *ff.*) The model is in many respects too neat, and tries to be too self-contained. But the evidence it presents for a dualistic Indo-European leadership, one religious, the other martial, is vast. It is, as Renfrew states, based on structural relationships. (p. 256) Such relationships need not be considered to be exclusively Indo-European, and if we find that they occur in other cultures it need not discredit their Indo-Europeanness, and we need not assume that they are entirely isomorphic functionally with their cultural correlates.

In neolithic settlement patterns, and in the wealth of neolithic and Copper Age temples, tomb-shrines, figurines, wall paintings, decorated vessels, and inscribed cult objects, inhere the Old European structures which we consider to be diametrically opposed to the Indo-European. Prior to 4500 B.C., we find a culture in which all life is sacred, and art a joyous and magnificent expression of that sacredness. Folklore and mythology support the view that social structure was not hierarchical, and where stratified was based on learned theocracy, not militant patriarchy. Villages and towns are not fortified as they are after the incursions. Like many of the cultures in the New World, Old European culture was focused on the harmonious interaction of humans in nature, and of men and women with each other as complementary, if not equal, partners. Symbols found on pottery and in temples are

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associated with water, or vegetation, or spirals of energy, and are often mixed and interchangeable. The Transcendent is manifest as the Goddess. Female figurines abound: main stereotypes include anthropomorphic birds (waterfowl and carrion-eating types) anthropomorphic snake women, pregnant women, women and frogs in birth-giving position, nursing mothers holding babies (humans, deer, and bears), and stiff white nude women ("Death", made of bone or alabaster).

After 4500 B.C., the quality of pottery in Balkan Europe declines sharply, hillforts and other fortified settlements become common, and symbols found are of axes, horses, men of war, and the sun. Some Old European cultures, like the Karanovo and Vinča and, later, the Cucuteni-Tripolye cultures, vanish with the coming of the Kurgans, and hybridized cultures, like the Cernavoda, Baden, and Coţofeni arise, appearing to take on Indo-European stratified social structure while preserving some measure of the indigenous religion and symbolic system. Baltic mythology is fully hybridized; in Celtic mythology we find an almost explicit account of the clash between the two symbolic systems: in the *Lebor Gabala Érenn*, in some of the wooing tales of CúChulainn, and in the Second Branch of the *Mabinogi*, questions of the rightful rulers of Ireland, and questions of the roles and power of women are treated, and goddesses throughout Celtic mythology are typically militarized. Indo-European gods, by contrast, are warriors, and are associated with the sun, the shining sky, thunder, lightning, and the sea. They are glorious warriors with shining swords, battle-axes, and magic steeds. In Indo-European thinking, great sacredness was given to the life of the elite; suttee burials with both human and animal victims testify to this radical stratification. Along the eastern Romanian border, where the first Kurgan incursions from the Pontic region ought to have occurred, a difference in the physical type of some members of the populations has been demonstrated. [Necrasov 1980] We may consider the influence of those first foreigners to have been social and cultural, as there does not seem to be evidence that massive groups of Pontic warriors conquered central Europe. [Schwidetzky 1980] Population

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movements of mixed physical types have been demonstrated for the period following the Second Wave of incursion at the time of transition between the Copper Age and Early Bronze Age. [Nemeskéri and Szathmáry 1987] It is only in the initial wave incursions into Europe that we find "pure" Indo-European structures; elsewhere the two strata are quickly mixed and assimilate to one another. In India, for example, it appears that the Dravidian substrate, which probably knew ascetic practices later becoming yoga, is driven down in favor of Indo-European Vedic religion until it resurges some centuries later. [Zimmer 1972:169-70 (*but cf. also* Varenne 1976:180)] I would not suggest that Indo-Europeans are all black-hatted cattle-stealing scoundrels, as some feminist writers might like us to think. Their priestly class was filled with men who were indeed learned: the wisdom of the druids was legendary in Caesar's time, and it is from First Function that the sages of Greece and the **witun* of the Germanic peoples must have sprung. The remarkable merging of the tradition of the Vedic brāhmanās with that of the indigenous yogis in India gave rise to one of the most advanced and vigorous philosophic and mystical cultures the world has ever known.

To deny the formal similarities among the symbols in Anatolia, the Balkans, Greece, Minoan Crete, and the megalithic cultures of Brittany and Ireland, with respect to the categorization of those symbols by Marija Gimbutas, is an onerous and unenviable burden. The reconstructed semantics of those symbols suggests an impressive semantic homogeneity of symbolism *throughout* Old Europe and persisting transformed into the historic period. To simply ignore this material as Colin Renfrew has done – rather summarily – is irresponsible. Perhaps he has felt that as an archæologist he cannot properly presume to evaluate religious materials, and perhaps its vagueness and unprovability has made him uncomfortable. It is true that this is prehistory, and we are endeavoring in reconstruction, an "archæology of mind". In such endeavors there comes a certain point where one *does* have to rely on intuition and opinion, and where great care must be taken. But it *can* be done. And its critics must at least acknowledge

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that their colleagues are making the attempt, especially when writing a book like this one, which, intended as it is for the layman, will serve only to confuse. It is more confusing still to compare this book with Renfrew's article in the Gimbutas *Festschrift* (*Proto-Indo-European: The Archaeology of a Linguistic Problem*, Ed. Edgar C. Polomé and Nancy Skomal). In this he determines that a number of incised clay cylinders from Sitagroi are *Old European*. [Renfrew 1987b:339] If we are to understand that Professor Renfrew accepts the existence of Old Europe *and* that he believes his theories in *Archaeology and Language* to be correct (both studies were published in 1987), we must accept his tacit implication that Old Europe was really Indo-European, and that the change we discover in symbol, social structure, and ideology following ca. 4500 B.C. was a total cultural transformation originating entirely from within the Indo-European community, and then spreading across Europe *as the traditional reconstruction of the Indo-Europeanization has it*. The evidence of the development of new cultures (Baden, Cernavoda, Coţofeni, Globular Amphora) [Gimbutas 1980a], as well as the evidence of physical anthropology cited above [Necrasov 1980, Schwidetzky 1980, Nemeskéri and Szathmáry 1987] make this compromise seem rather untenable. It slips too far over the edge of Ockham's razor.

The Indo-European problem is not what it used to be. It has grown, and it is time to restate its parameters for new investigation. Where is the primeval Homeland? Where are the secondary and tertiary Homelands in Europe? If the cultural unity which is called the Globular Amphora Culture (north central Europe, ca. 3400-3000 B.C., following the 2nd Wave of incursions into Europe [Gimbutas 1980a]) is taken to be one of them, might there have been there a dialectal unity as well? If we discover such unities, can the dialect, say, of a pre-Germano-Baltic, be reconstructed? What ramifications will those *intermediate* dialects have on our reconstruction of proto-Indo-European, and of the interpretation of our analyses of the archæological record? (*cf.* Lehmann [1987:84-85])? What are the factors responsible for the Indo-Europeanization of Europe? If it is in fact not a question of invasion and force of

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arms, then what social, economic, and/or ideological factors might be responsible for the adoption of the patriarchal system over the indigenous gynandric one? What might be a convincing scenario for the *peaceful* Indo-Europeanization of, say, an indigenous megalithic community? What are the criteria for discovering which facets of the mixed cultures are substrate Old European, and which are Indo-European proper? If we can differentiate, will our discussions of what's going on in, for example, La Tène be more fruitful than they have been hitherto? What can we say about the worldview and religion of typical (if hypothetical) Old European or Indo-European *individuals* given our reconstructions of their religion from our symbolic analyses? What are the lessons of Old European and Indo-European history? How does what we know about the Christian tradition and its triumph in Dark Age and Medieval Europe fit in, and how is our understanding of the process of symbolic and ideological tenacity and preservation altered by such investigation? When we learn about who our ancestors were, and how they got where they did, what do they teach us about who *we* are? How do we proceed from what we know of the past into what we hope to create in the future?

A good, popular, practical synthesis of what is known about Old European and Indo-European history is to be greatly desired. Colin Renfrew's edition is the only major work to have been attempted recently, which is a great pity, for the vision of his revisionist theory is nowhere near as plausible or as elegant as is the more traditional approach which he decries. In his chapter on the Celts (pp. 211-50) there is much good in the way of synthesis and analysis, if not too much in the way of novelty. The agriculturalization of Europe (ca. 7000- 6500 B.C. onward) transformed completely the way of life of the population of Europe. There is no evidence to suggest that the fundamental realities of their religion – the Transcendent and Immanent Goddess manifest in her functions of life-creation, increase, death-wielding, and *regeneration* – was substantially changed in more than focus: *there is continuity of these basic religious ideals and their concomitant symbols for some 20,000 years* from the Upper Palæolithic through the

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Neolithic. Therefore the transformation of culture which began ca. 4500 B.C. with the first incursions of the bearers of Indo-European language and Indo-European ideology can be taken as one of the greatest transformations in human consciousness and history. Such a transformation may not be ignored, and deserves a thorough and inspired treatment which will inspire further investigation and engender a genuine understanding of the history of humankind. Sadly, Colin Renfrew's *Archaeology and Language* is not such a book, being an overly hasty essay in theory and rhetoric which does not serve such investigation.

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BOOK REVIEWS

THE FISHING CULTURES OF THE WORLD

Ed. B. Gunda
Akademiai Kiado, Budapest

The volume includes papers of outstanding ethnologists, social and cultural anthropologists, archaeologists and linguists predominantly in English, but also in French and German, on the fishing of the five continents and on issues connected with fishing (*e.g.* the form of boats). The authors discuss fishing ecology, the way of life of fishermen, the organization of their work, the historical strata and classification of the fishing implements, the mythology of fish and fishing. The papers throw light upon the manner of living of fishermen of Spain, Sweden, Italy, as well as of the various islands in the Pacific. Rich fishing traditions of the Far East, the salmon fishery and fishermen's mythology of the Indians of North America are also discussed.

Special stress is laid on presenting the geographic range, cultural trends and historical evolution of fishing cultures, and some of the authors emphasize the significance of the ecological factors which shape the development of different fishing communities.

Published in two volumes, there is little pattern to the collection, which contains some sixty different manuscripts, taking up more than 1200 pages, but the variety and wealth of data more than makes up for any absence of methodological or theoretical unity. Religion in fishing communities is well covered (the very first article deals with fish in the religion and myths of ancient Mesopotamia), as is the technology of fishing, the social structure of fishing communities, prehistoric, early historic and contemporary fishing communities, and the problem of acculturation facing traditional fishing communities in their effort to accept new technology.

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A PEOPLE OF THE SEA
THE MARITIME HISTORY OF THE CHANNEL ISLANDS
Ed. A.G. Jamieson

Throughout their history, the Channel Islands have depended on the sea. In medieval times the Islanders played an important part in Anglo-French trade and during the age of sail the merchant ships of Guernsey and Jersey travelled to the far corners of the world. From the late sixteenth century to the early 1900s the islanders were experienced in most aspects of the international shipping industry.

With the demise of the sailing ship in the early twentieth century the Islands' participation in seaborne trade decreased but the local economy remained dependent on shipping, particularly the cross-channel steamers.

A People of the Sea records the varied and intriguing maritime past of the Channel islands and their inhabitants. Beginning with prehistoric times the book draws on records from Britain, France, Canada and the Islands themselves to piece together a unique and fascinating history. With eight contributions by mainly British authors, this handsome volume contains in excess of five hundred pages, the papers being illustrated by about sixty color and black and white plates, as well as an adequate supply of maps.

Commencing with the eighth millennium B.C., the compilation embraces virtually every aspect of maritime history discernable. There are excellently detailed and fascinating papers on the history of Channel Isles merchant shipping and trade, shipowners and seamen, privateering, smuggling, shipbuilding, fishing, overseas settlement, maritime strategy and harbor development.

This comprehensive and authoritative study does not view the maritime history of the Channel Islands in isolation, but relates it to international maritime developments: the general expansion of the European maritime powers into the wider world, particularly the rise and decline of the British seaborne empire, also affected the Channel Isles and is not neglected.

Since the Second World War the Islanders' relationship

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with the sea has inevitably been altered. Finance, tourism and agriculture have brought great prosperity to the Islands in the face of ever decreasing maritime activities. However, although few Islanders now derive their living from the sea, one can still detect something of the days when they could be described, as Victor Hugo did, as "a noble little people" who "possess the spirit of the sea."

DEVELOPING AMAZONIA: DEFORESTATION AND SOCIAL CONFLICT
BRAZIL'S CARAJAS PROGRAMME
Anthony L. Hall
Manchester University Press

The Grande Carajas programme was established by the Brazilian government in 1980 and covers an area the size of Britain and France combined. As the latest and by far the largest official scheme seeking systematically to tap the mineral, forest and agricultural potential of Amazon its most controversial aim is to introduce iron and steel production into the region, utilizing the native rain forest as a source of charcoal to fuel the smelters

Developing Amazonia focuses on the Carajas programme as a case study of the tremendous social costs of this predatory pattern of occupation, which, the author claims will bring little lasting benefit to the majority of those who live and work in Amazonia, apart from the vast ecological damage that will ensue. After placing the Carajas programme within the general context of official Amazonian development strategies, the book examines the specific social costs associated with the emerging crisis in the region: rural violence and intimidation, land-grabbing, polarisation of land ownership, landlessness, forced population displacements, encroachment upon Indian territory and environmental decay. It goes on to describe growing popular resistance by peasant farmers in their struggle for a livelihood and concludes by drawing lessons from the Carajos experience for both Brazilian and global tropical rain forest development.

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